NBER WORKING PAPER SERIES

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Working Paper 31142
http://www.nber.org/papers/w31142

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2023

This work was supported by the National Institutes of Health [grant numbers P50DA046351, R21DA04595]; and the Centers for Disease Control and Prevention [grant number 5R01CE002999-02]. Funders had no role in study design; data collection, collection, and analysis; manuscript preparation; or the decision to submit the article for publication. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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Investigating the Complexity of Naloxone Distribution: Which Policies Matter for Pharmacies and Potential Recipients

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NBER Working Paper No. 31142
April 2023
JEL No. H75,I18,K32

ABSTRACT

Despite efforts to address the opioid crisis, opioid-related overdoses remain a significant contributor to mortality. State efforts to reduce overdose deaths by removing barriers to naloxone have recently focused on pharmacy channels, but the specifics of these laws and the contexts in which they are implemented vary widely. In this paper, we use novel methods robust to heterogeneous effects across states and time-varying policy effects to estimate the effects on naloxone pharmacy distribution of two types of laws: laws authorizing non-patient-specific prescription distribution of naloxone and laws granting pharmacists prescriptive authority for naloxone. We find that both types of laws significantly increase the volume of naloxone dispensed through pharmacies. However, relative to laws authorizing non-patient-specific prescription distribution, effects are significantly larger for pharmacist prescriptive authority laws. These larger effects only partially derive from increased naloxone prescribing by pharmacists. We also estimate large, significant increases in pharmacy dispensation of naloxone prescribed by non-pharmacist prescribers, with particularly large increases among family medicine physicians, with particularly large increases among family medicine physicians. The relative benefits of pharmacist prescriptive authority laws versus non-patient-specific distribution are larger among Non-Hispanic Black individuals, suggesting an important role of these policies for reducing disparities in access to naloxone.

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1. Introduction

Improved access to naloxone is one of the central pillars of federal, state, and local policy responses to the opioid overdose crisis, as indicated by the March 29, 2023, decision by the FDA to allow over-the-counter sales of Narcan, a specific spray formulation of naloxone (FDA, 2023). In 2021, over 100,000 people died from drug overdose, most of which involve opioids (CDC, 2022). Administering naloxone to a person overdosing from opioids can reverse the effects of the overdose if administered in time, and the medication is non-addictive with minimal side effects (Cawley & Dragone, 2023; Chamberlain & Klein, 1994). While naloxone is increasingly carried by first responders (Smart et al., 2022) and local community groups (Clark et al., 2014; Wheeler et al., 2015), continued gaps in naloxone availability prompted policy interest in improving naloxone access for bystanders by dispensing more naloxone through pharmacies (ASPE, 2021; CDC, 2019; Guy et al., 2019). To facilitate access to this lifesaving medication, every state had adopted some type of naloxone access law (NAL) to remove barriers to obtaining naloxone through pharmacies by 2018 (Smart et al., 2021).

There is mixed evidence regarding the impacts of these state NALs on fatal overdoses (Abouk et al., 2019; Doleac & Mukherjee, 2022; Erfanian et al., 2019; Lee et al., 2021; McClellan et al., 2018; Rees et al., 2019; Rudolph et al., 2022). Potential explanations for these mixed findings are that NALs vary in how they reduce the monetary or non-monetary costs of obtaining naloxone (Smart et al. 2021; Davis & Carr, 2015). Past studies have differed in how they classify the laws; and differences in study timeframes result in a disparate set of states and laws contributing to identification. Furthermore, there is limited work assessing how NALs affect naloxone dispensing—the first-order outcome targeted by these policies. While some studies suggest that NALs increase pharmacy distribution of naloxone (Smart et al., 2021),

questions have been raised about which aspects of NALs are most effective at achieving this goal. A body of implementation research has also raised questions about the extent to which NALs are capable of increasing naloxone distribution given lack of knowledge among pharmacists about their state NALs (Thakur et al., 2020) and failure to stock naloxone in pharmacies (Abbas et al., 2021; Eldridge et al., 2020; Graves et al., 2019; Meyerson et al., 2018; Spivey et al., 2020). Finally, one criticism of pharmacy-focused laws is that they may not be effective at helping vulnerable or disadvantaged populations who face stigma from prescribers and pharmacists (Green et al., 2020; Smart & Grant, 2021) and for whom the often-substantial price of naloxone (Peet et al., 2022) may represent a serious access barrier. However, no study has evaluated differential effects of NALs on naloxone receipt across population subgroups.

This paper evaluates how NALs influence this first-order outcome of pharmacy-based naloxone distribution. We jointly estimate the effects of two types of laws that aim to reduce non-monetary consumer costs associated with obtaining a naloxone prescription prior to visiting a pharmacy: (1) non-patient-specific (NPS) prescription distribution models, which include protocol orders, standing orders, and collaborative practice agreements; and (2) pharmacist prescriptive authority. In states with NPS prescription distribution, pharmacists (and sometimes community-based organizations) can dispense naloxone to any individual who meets certain criteria specified by a designated non-pharmacist prescriber or medical licensing board (Davis & Carr, 2015). In prescriptive authority states, pharmacists are granted the ability to prescribe naloxone directly to patients. Both types of law obviate the need for individuals seeking naloxone to obtain a prescription prior to visiting a pharmacy. However, the two types differentially affect the procedures required of pharmacies and pharmacists in ways that may

differentially shape expected costs incurred by pharmacies and pharmacists, and therefore their willingness to stock and dispense naloxone.

As all states now have at least one of these pharmacy-based naloxone distribution laws in place, it is useful to evaluate whether one type of policy impacts naloxone access more than the other. It is also useful to evaluate whether the policies differ in *who* benefits from improved access. We thus estimate relative effects on total naloxone pharmacy distribution for these two law types, but we also consider differential effects across health care providers and the extent to which different policies disparately impact naloxone access for different demographic groups. For all results, we estimate effects using a difference-in-differences framework that produces valid average treatment effects in the context of policy effect heterogeneity, extending the imputation-based approach of Gardner (2022) to accommodate multiple policies. We explore the sensitivity of the proposed approach to alternative assumptions; we also leverage information from two different largescale sources of pharmacy data, demonstrating that our findings replicate across both.

We find that both pharmacist prescriptive authority and NPS prescription laws significantly increase the volume of naloxone dispensed through pharmacies. However, effects are significantly larger for pharmacist prescriptive authority laws. These larger effects only partially derive from increased naloxone prescribing by pharmacists; we also estimate large, significant increases in naloxone fills for other prescriber types, with particularly large increases among family medicine physicians and physician assistants. These differential effects have implications for addressing naloxone access gaps among disadvantaged populations. While we find increases in prescriptions filled by patients from all racial/ethnic, sex, and age groups in response to both NPS prescription laws and pharmacist prescriptive authority laws, Non-

Hispanic Black individuals benefit statistically significantly more from pharmacist prescriptive authority laws than from NPS prescription laws.

Our paper makes three main contributions. First, using information on the recorded naloxone prescriber, we are able to directly assess one of the purported mechanisms through which specific types of naloxone laws work. Specifically, while previous work has suggested that NALs granting prescriptive authority to pharmacists may be disproportionately effective in increasing pharmacy naloxone distribution compared to NPS prescription laws (Abouk et al., 2019), we provide further evidence supporting the causal effect of these policies by evaluating the specific role that pharmacists play in prescribing naloxone in these states. We also assess possible substitution patterns across prescriber types or whether these policies increase naloxone dispensing more generally. We thus provide initial evidence of the extent to which pharmacists' prescribing of naloxone might "crowd out" naloxone prescribing by others or, conversely, whether granting pharmacists prescriptive authority has spillover effects in increasing pharmacist dispensation of naloxone that has been prescribed by other providers.

Second, we assess whether the two types of NALs disparately impact naloxone access for different demographic groups. While there has been broad interest in understanding the demographics of the opioid crisis (Alexander et al., 2018; Altekruse et al., 2020; Ho, 2017; Om, 2018; Powell, 2021; Shiels et al., 2018; Tipps et al., 2018), there is little evidence about the demographics of individuals who receive naloxone at the pharmacy. Ours is the first study to evaluate whether NALs allowing different pharmacy-based distribution models increase naloxone distribution for particular subpopulations, potentially exacerbating or mitigating disparities in naloxone access. This is an important consideration given evidence that racial/ethnic minority groups face greater difficulties in access to harm reduction services

(Rosales et al., 2022), as well as inequitable access to treatment and other health care system resources (Yearby, 2018).

Finally, this study is the first in the NAL literature to address concerns about bias due to the staggered implementation of these policies (Goodman-Bacon, 2021), despite substantive concerns about policy effect heterogeneity due to variation in the development of the opioid crisis, differences in the specific policies that comprise states' NALs, and evidence of implementation lags in pharmacy responses to policy changes. We address these issues by implementing and extending an approach that imputes counterfactual "untreated" outcomes for treated observations to estimate treatment effects (Gardner, 2022). Much of the emerging "new" difference-in-differences literature is difficult to apply to a setting with multiple types of policies. In our context, the two policies of interest can be modelled as non-overlapping, which simplifies some of the complexities of jointly estimating two difference-in-differences models and permits use of the imputation approach. We introduce a straightforward extension to estimate average treatment effects for these situations.

Given the ongoing surge in drug-involved fatalities, driven now by synthetic opioids, a clearer understanding of the specific mechanisms through which naloxone policies work to increase pharmacy distribution of naloxone is critical. Which policies work? Who do they help? These questions remain relevant even with the most recent FDA decision to allow Narcan to be sold over the counter, as they speak to the potential effects of reducing cost-related barriers for naloxone dispensers (e.g., information and time costs) and patients (e.g., monetary and time costs) through retail channels. Furthermore, our findings emphasize the importance of attending to pre-existing state NAL environments and demographic differences as likely moderators of the impacts of the FDA decision. We provide important evidence to help inform the contribution of

various types of NALs toward expanding naloxone pharmacy distribution and to better conceptualize the likely implications of federal changes in naloxone regulations.

2. Background on Naloxone Access Laws and Their Effects

In 2001, New Mexico became the first state to adopt a naloxone access law (NAL) aimed at increasing naloxone availability to individuals who are at increased risk of experiencing or witnessing an opioid overdose. The timing of this state law coincided with the launch of the first large-scale take-home naloxone prescription program in the United States, implemented in a rural New Mexico county that had the highest heroin overdose mortality rate in the country (Burris et al., 2001). Over the next decade, five more states passed similar legislation. Many of these early NALs removed potential liability for prescribing, dispensing, or administering naloxone but still required a patient-specific prescription to purchase naloxone at a pharmacy. While these protections from civil or criminal liability in theory reduce the legal costs associated with providing or using naloxone, the actual liability risk of prescribing or dispensing naloxone in accordance with state law is minimal (Davis & Carr, 2017). These early laws did little to reduce the non-monetary costs incurred by individuals seeking to obtain naloxone, however, because individuals still had to obtain their own patient-specific prescription. This may represent a serious barrier to individuals who use opioids. Given systemic stigma and lack of access to traditional health care systems, people who misuse opioids or know people who misuse opioids may not be willing to see a physician or may not have the time to see a physician before they anticipate needing naloxone.

In more recent years, states have adopted NALs aimed at removing this potential access barrier by permitting pharmacists to dispense naloxone through non-patient-specific prescription models (e.g., standing orders) or through pharmacist prescriptive authority. Illinois was the first state to adopt a non-patient-specific prescription model of naloxone distribution in 2010, implemented via standing order. In April 2014, New Mexico became the first state to grant pharmacists prescriptive authority, 13 years after the state's initial NAL passed. By the second half of 2018, all states had implemented at least one of these models, with most states allowing for non-patient-specific prescription models rather than pharmacist prescriptive authority (see Figure 1).

The mechanism of action for both models of naloxone distribution is to obviate the need for individuals seeking naloxone to obtain a prescription prior to visiting a pharmacy, substantially reducing potential time and monetary costs associated with obtaining naloxone. From the naloxone recipients' perspective, these laws may look functionally equivalent in how they reduce non-monetary costs of obtaining naloxone—in both cases, an individual can simply present to the pharmacy and receive the medication (potentially conditional on payment or receipt of training and education). However, the models differ in the processes required of prescribers and pharmacists (Green et al., 2015). In states with laws authorizing non-patient-specific prescription models (hereafter referred to as "NPS prescription laws"), pharmacists (and sometimes community-based organizations) can dispense naloxone to any individual who meets certain criteria specified by a non-pharmacist prescriber or medical licensing board. In some states, these laws permit any authorized prescriber to issue a standing order for naloxone distribution, which often requires effort on the part of pharmacies and pharmacists to find a collaborating prescriber (Green et al., 2015); in other states, the laws direct a state government

official to issue a standing order or comparable directive that applies to all entities in the state. In both cases, directives can vary in formulations of naloxone authorized, scope of population covered, and training or educational requirements for the pharmacist dispenser or naloxone recipient (Davis, 2020). Decisions around the specifics of these directives often take time, sometimes leading to notable lags between the effective date of the NPS prescription law and the signing of the order (e.g., see Mozingo, 2018).

Laws granting pharmacists prescriptive authority are less heterogeneous in that they all grant pharmacists the ability to prescribe naloxone directly to patients. Thus, there is a patient-specific prescription, but an individual need not obtain a prescription before entering the pharmacy because the pharmacist acts as prescriber. Because these laws directly expand pharmacists' scope of practice and commonly impose pharmacist training requirements (Roberts et al., 2019), pharmacists may be more likely to know about their adoption relative to NPS prescription laws. This is a potentially important distinction because several studies of states with NPS prescription laws have shown that a substantial percentage of pharmacists are unaware that they can dispense naloxone without a patient-specific prescription or exhibit inaccurate beliefs about who can be dispensed naloxone under the standing or protocol order (Evoy et al., 2018; Santa et al., 2021; Thakur et al., 2020). Perhaps partially due to these information barriers, studies in several states with NPS prescription laws have found that a high percentage of pharmacies fail to stock naloxone (Carpenter et al., 2019; Eldridge & Meyerson, 2020; Evoy et al., 2018).

In addition, by explicitly allowing pharmacists to act as naloxone prescribers, prescriptive authority laws may further reduce pharmacists' actual or perceived costs associated with dispensing naloxone. By more directly involving pharmacists in naloxone access efforts,

pharmacist prescriptive authority laws may mitigate pharmacists' concerns about dispensing naloxone under a NPS prescription model (in states that allow NPS prescription distribution and pharmacist prescriptive authority), enhance pharmacists' confidence around naloxone and thus increase their willingness to dispense the medication, and improve accuracy of processes for billing insurance for dispensation under a standing or protocol order (Evoy et al., 2018; Santa et al., 2021; Thakur et al., 2020). To the extent that engaging pharmacists more directly in harm reduction efforts reduces stigmatizing beliefs about individuals seeking naloxone (Santa et al., 2021), these policies may also address discriminatory behavior by pharmacists that results in inequitable naloxone distribution across age, race/ethnicity, or community characteristics.

The extent to which these different types of laws produce differential effects on naloxone distribution and, accordingly, on population-level health outcomes, is unclear. While a small literature has evaluated the effects of NALs on naloxone distribution, most studies have either focused on laws mandating naloxone co-prescription with high-dose opioids (Green et al., 2020; Sohn et al., 2019) or have evaluated early iterations of NALs prior to the expanded adoption of pharmacist prescriptive authority laws (Gertner et al., 2018; Xu et al., 2018). Abouk et al. (2019) evaluated the effects of "direct authority" NALs, which they defined as state policies providing pharmacists explicit authority to dispense naloxone without a prescription or granting pharmacists prescriptive authority. They found statistically insignificant effects of these laws, as well as "indirect authority" NALs, on naloxone prescribing. However, their analysis was restricted to Medicaid naloxone prescribing and was likely underpowered given the noisiness of those data, and the implementation features that accurately distinguish "direct authority" and "indirect authority" are somewhat unclear (Hill et al., 2019).

Most closely related to our current work is a study by Xu & Mukherjee (2021) that evaluated the effects of pharmacist prescriptive authority laws on naloxone dispensing. Using all-payer pharmacy data from 2010 to 2018, they find a significant 53 percent increase in naloxone prescriptions dispensed following prescriptive authority adoption, although the dynamic event study estimation of these effects suggests there may be problematic pre-trends, and the post-policy effects may not persist more than one year post-implementation. However, the authors were unable to examine the extent to which these effects are driven by pharmacist-written prescriptions, nor whether these effects are distributed equitably across population subgroups. Additionally, their approach may produce biased estimates of policy effects in the context of policy effect heterogeneity (Sun & Abraham, 2021).

3. Data

To estimate the effects of naloxone access laws on pharmacy distribution of naloxone, we combine data from several sources over the period of 2010 to 2018. Our outcome and policy data are defined at the quarterly level, while our covariate data are quarterly or annual.

Data on state naloxone access laws come from the Prescription Drug Abuse Policy System (PDAPS), supplemented by original legal research.[1] Specifically, we identified effective dates for: (1) laws that grant pharmacists prescriptive authority for naloxone; (2) laws authorizing dispensing of naloxone by pharmacists without a patient-specific prescription; or (3) NALs that do not allow pharmacists to dispense naloxone without a patient-specific prescription

[1] In cases where our legal research disagreed with information in PDAPS, we contacted PDAPS to resolve the discrepancies. In these cases, PDAPS subsequently changed their dates to align with our interpretation; these changes are documented in PDAPS's protocols and reflected in the updated release of its data in January 2022.

from another provider.[2] While NALs vary in many ways, we focus on the former two dimensions because they directly address important non-monetary costs[3] of obtaining naloxone for individuals who may not regularly engage with a healthcare provider or who may not be able or willing to obtain a naloxone prescription from a provider prior to visiting a pharmacy. However, all models control for whether a state has some other form of NAL (e.g., a law providing criminal or civil liability protections for those who prescribe naloxone), and we show in supplemental analyses that these other forms of NALs have little if any effect on naloxone distribution through pharmacies (see Figure A3 below). Table 1 lists the effective dates of the relevant legislation for all states.

We use two complementary sources of data regarding pharmacy distribution of naloxone, with naloxone prescription fills identified using National Drug Codes. To analyze effects by prescriber type, we use data from the IQVIA Real World Data Longitudinal Prescriptions dataset, which contains information on an estimated 92% of all prescriptions filled at retail pharmacies in all 50 U.S. states and the District of Columbia for all payers, including Medicare, Medicaid, third party, and cash payers, as well as limited information on patient demographics, provider specialty, and pharmacy location. These data are particularly helpful because IQVIA links prescriber specialty information to the prescription data, making it possible to identify the type of prescriber (family medicine physician, internal medicine physician, pain physician, nurse

[2] Oklahoma is difficult to categorize within this typology as the statute effective in November 2014 (Ok. Stat. Ann. tit. 63 § 2-312.2) directly authorized pharmacists to dispense naloxone without a prescription (and without a standing order), which is not permitted by federal law. Subsequent changes to the law in 2017 clearly grant pharmacists prescriptive authority. We test the sensitivity of treating the 2014 law as NPS prescription distribution and 2017 law as pharmacist prescriptive authority (done in the main analysis) by (1) redefining Oklahoma as adopting pharmacist prescriptive authority in November 2014, and (2) dropping Oklahoma from the analysis. The estimates, presented in Figures A9 and A10 below, are similar to the main estimates.

[3] The policies also potentially address the total costs of obtaining naloxone by removing the need to see a physician, which often carries monetary costs in addition to time costs.

practitioner, physician assistant, and so on) who prescribed the naloxone being dispensed. We aggregate these transaction-level data to cells defined by state, year-quarter, and prescriber type. For our main analysis, we consider all naloxone claims. We then stratify based on whether a pharmacist was the issuing prescriber. Finally, we study claims for several different prescriber types including nurse practitioners, physician assistants, family medicine physicians, internal medicine physicians, and pain medicine physicians. We convert naloxone fills to per capita rates using population data from the Surveillance, Epidemiology, and End Results (SEER) Program, which modifies published Census data (National Center for Health Statistics, 2021).

To analyze effects by patient characteristics, we use naloxone pharmacy distribution data from Symphony Health because it provides richer information on prescription recipient characteristics. Similar to the IQVIA data, the Symphony Health data describe a 92 percent sample of prescriptions filled at retail pharmacies. These data were aggregated and provided to us in cells defined by 3-digit zip code, year-quarter, and the following demographic characteristics: race/ethnicity, age category, and sex. For race/ethnicity, we have data for non-Hispanic White, non-Hispanic Black, Hispanic, and Other. For age categories, we observe dispensing for ages 0–18, 19–38, 39–58, and 59+. For confidentiality reasons, we only observe one demographic type at a time (i.e., we observe naloxone dispensing by age group, state, and quarter but do not know race; we separately observe naloxone dispensing by race, state, and quarter). Thus, we are unable to study any interactions between demographic groupings. We scale claims using population data from SEER.

Claims data sets typically do not include such rich demographic information, but Symphony Health data are merged with third party data (e.g., shopper cards, specialty pharmacy data, media, etc.) to determine, when possible, age, sex, and race/ethnicity. Symphony Health is

not always able to identify this information for all claims. The rate of unknown information varies by characteristic. For example, sex is never missing, while race/ethnicity is missing for 36 percent of claims. We assume that NAL adoption is not correlated with the missing rate, and we test this assumption by studying how the rate of missing responds to policy changes; results support that our findings for race/ethnicity do not reflect effects of the laws on improving completeness of the demographic information (see Figure A1).

Finally, covariate data are drawn from several sources. State-level demographic information are collected from SEER. We include share White and non-Hispanic, as well as five variables measuring the age composition of state residents (share aged 0–17, 18–34, 35–44, 45–54, and 55–64). We also condition on several state policy variables that were implemented throughout the same period. Information on adoption of state ACA Medicaid expansion (Abouk et al., 2021), pain management clinic laws, active and legal cannabis dispensaries, mandatory-access PDMPs, and Good Samaritan Laws were all available through the RAND-USC Schaeffer OPTIC Policy Database (OPTIC, 2022).

4. Empirical Strategy

We implement a difference-in-differences strategy, comparing quarterly naloxone claims per 100,000 population in adopting states to those in non-adopting states both before and after policy implementation, using methods that can appropriately accommodate heterogeneous treatment effects. The standard two-way fixed-effect (TWFE) estimators used by previous difference-in-differences studies of NAL effects may produce biased treatment effect estimates in the presence of staggered adoption and treatment heterogeneity due to the implicit use of early-adopters as controls for late-adopters (Sun and Abraham, 2021). If the policy effect grows (or attenuates) over time but this dynamic effect is not properly modeled for the early-adopters

specifically, those dynamic treatment effects become part of the control for the later-adopters and thus create a "contaminated" control group.

Given concerns about bias due to the interaction of staggered implementation and treatment heterogeneity, we implement an extension of two-stage difference-in-differences (2sDID; Gardner, 2022), an imputation approach that circumvents these concerns.[4] This method models untreated outcomes as a function of state fixed effects, time fixed effects, and covariates, generating parameter estimates using only untreated observations (first stage). Counterfactual outcomes of the treated observations are then imputed based on the parameters estimated in the first stage, with treatment effects then defined as the difference between the observed and counterfactual outcomes for the treated observations. Unlike standard TWFE regressions, this imputation-based approach avoids the problem of "contaminated controls" by imputing counterfactuals based only on untreated observations. We follow Gardner (2022) by conducting estimation in a GMM framework, which permits straightforward estimation of standard errors (adjusted for state-level clustering) to account for the two-step process (Hansen, 1982).

The original application of Gardner (2022), as well as many of the new difference-in-differences methods robust to treatment heterogeneity, consider the case of identifying the average effect of a single policy variable.[5] In our context, we are interested in studying two separate dimensions of NALs, but the policies can be considered non-overlapping. This property provides a straightforward way to extend the imputation approach for our analysis.

[4] Borusyak et al. (2022) introduce a related imputation estimator. When the interest is estimation of the average treatment effect (overall or for a specific time period), the estimators are identical.
[5] de Chaisemartin & d'Haultfoeuille (2022) represent an exception that identifies the immediate (but not dynamic) impact of each policy given multiple policies.

We consider state outcomes untreated if the state has not adopted an NPS prescription law or pharmacist prescriptive authority law. We designate untreated outcomes for unit *s* at time *t* as $Y_{st}(0,0)$, where the first index refers to NPS prescription laws and the second index refers to pharmacist prescriptive authority (equal to 0 if the policy is not in place and 1 otherwise). We model

$$Y_{st}(0,0) = \alpha_s + \gamma_t + \boldsymbol{X}'_{st}\boldsymbol{\beta} + \varepsilon_{st}, \qquad (1)$$

where α_s represents the state fixed effect, γ_t represents the time (year-quarter) fixed effect, and $\boldsymbol{X}_{st}$ is a vector of state- and time-varying predictors. This vector includes the share of the state population that is White and non-Hispanic, five age share variables, and the policy variables mentioned above.

The treatment effect for NPS prescription laws is the population-weighted average of outcomes observed with an NPS prescription law in place minus the imputed values for those observations. Let $\mathcal{S}$ represent the set of state-quarters with NPS prescription laws in effect, $P_{\mathcal{S}}$ designate the total population of these observations, and w_{st} represent the population of state *s* at time *t*. The treatment effect then for NPS prescription laws is defined as:

$$\hat{\delta}_1 = \frac{1}{P_{\mathcal{S}}} \sum_{(s,t)\in\mathcal{S}} w_{st} \left(Y_{st}(1,0) - \hat{Y}_{st}(0,0) \right) \qquad (2)$$

The pharmacist prescriptive authority treatment effect is defined comparably for the set of state-quarters with pharmacist prescriptive authority NALs, represented by $\mathcal{D}$:

$$\hat{\delta}_2 = \frac{1}{P_{\mathcal{D}}} \sum_{(s,t)\in\mathcal{D}} w_{st} \left(Y_{st}(0,1) - \hat{Y}_{st}(0,0) \right) \qquad (3)$$

We also present the equivalent event study estimates defined by year-relative-to-adoption. These estimates help us assess whether the "parallel trends" assumption held prior to adoption and to

evaluate any dynamic treatment effects. To improve precision, we aggregate the event study estimates to the annual level. Let t_S represent the quarter of adoption for the state's NPS prescription law and t_D represent the time of adoption for direct authority. The event study estimates are indexed by *k*, year relative to adoption. As an example, define $\mathcal{D}_{k=1} \equiv \{(s,t) \mid 0 \leq t - t_D < 4\}$, which groups the first four quarters post-adoption into a "first year after adoption" estimate. The event study estimates, then, are defined by

$$\hat{\delta}_{2,k} = \frac{1}{P_{\mathcal{D}_k}} \sum_{(s,t)\in\mathcal{D}_k} w_{st} \left(Y_{st}(0,1) - \hat{Y}_{st}(0,0) \right). \quad (4)$$

For NPS prescription laws, equation 3 above implicitly selects on states that have not yet adopted pharmacist prescriptive authority laws as we consider states as not having NPS prescription laws upon adoption of pharmacist prescriptive authority. The same convention applies to the event study estimates such that, for example, $\mathcal{S}_{k=1} \equiv \{(s,t) \mid 0 \leq t - t_S < 4 \;\&\; t - t_D < 0\}$. Thus, the event study estimates for NPS prescription laws do not use any observations from state-quarters with pharmacist prescriptive authority.

The difference-in-differences estimates, defined in equations 2 and 3, should not be affected by jointly studying two dimensions. However, the pre-treatment event study estimates for the pharmacist prescriptive authority dimension refer only to a subset of the states adopting pharmacist prescriptive authority NALs. For example, by selecting on states without NPS prescription laws, we exclude state-quarters that have NPS prescription laws in place prior to adopting pharmacist prescriptive authority*:* $\mathcal{D}_{k=-1} \equiv \{(s,t) \mid -4 \leq t - t_D < 0 \;\&\; \text{NPS} = 0\}$. This affects North Dakota and Oregon.[6] Since these states are "treated" (by NPS prescription

[6] Maine adopted pharmacist prescriptive authority in June 2018, after our sample period, and is not considered "treated" in our main analysis.

policy) prior to adoption of pharmacist prescriptive authority, we should not expect the pre-(pharmacist prescriptive authority)-adoption estimates to be zero, and thus they are not informative about pre-existing trends.[7]

For NPS prescription laws, we present pre-adoption event study estimates relative to 4+, 3, 2, and 1 year prior to adoption (note that imputation approaches do not require normalizing the pre-period estimates). We also present estimates for the year of adoption (year 0) and years 1, 2, and 3+ after adoption. For prescriptive authority, we present estimates referring to the same time-relative-to-adoption time periods, except that the last estimate is for 2+ since we would only observe one state (New Mexico) in the 3+ bin. The "first step" implicitly normalizes all estimates to the average of the pre-period. All steps in the analyses are population-weighted (for the relevant demographic group). Standard errors are adjusted for the two-stage process and for clustering at the state-level.

While we have data through 2018, our imputation-based approach requires untreated observations to identify the state fixed effects and the time fixed effects. All states adopted one of these policies by 2018q3, implying that the subsequent time fixed effects are not identified. However, even prior, the time fixed effects are identified from only one untreated state: Nebraska.[8] To reduce the potential leverage of a single state driving estimates of the

[7] The existence of differential pre-trends in states adopting pharmacist prescriptive authority on top of NPS prescription could reflect endogenous adoption (threatening causal identification) or dynamic effects of NPS prescription laws (not threatening causal identification), and we cannot disentangle these.

[8] Nebraska is particularly worrisome as the only control state because there is ambiguity about whether the state should be considered as having an NPS prescription law or not. While Nebraska does not have a statute authorizing NPS prescription distribution by standing order or otherwise, the state has had a statewide standing order drafted and posted since at least August 2018 (see current version of the order at https://dhhs.ne.gov/DOP%20document%20library/Naloxone%20standing%20order.pdf). The statute referenced within the standing order document as allowing dispensation without a prescription does not in fact do so.

counterfactual, we truncate the sample for our analysis at 2017q2 such that there are always at least three states identifying the time fixed effect. In a complementary analysis, described in Section 6, we present results for pharmacist prescriptive authority law states relative to NPS prescription law states using the full time series.

We first present the overall effects, followed by prescriber-specific effects then demographic-specific effects. We then report results from a series of sensitivity analyses for the overall effects, relying on validation of the identifying assumptions in the overall sample as suggestive that any potential biases are not impacting the results for any specific subgroup. When presenting difference-in-differences results, we test the equality of the NPS prescription and prescriptive authority estimates and present the p-value from this test. We also include the counterfactual mean, which we define as the average of the outcome variable for observations with pharmacist prescriptive authority after adjusting for the causal impact of the law. We subtract off the prescriptive authority estimate and then calculate the weighted average of this counterfactual for the prescriptive authority observations. In principle, this is the value of the outcome observed if the state had not implemented pharmacist prescriptive authority. We also present the equivalent counterfactual for NPS prescription laws.

5. Results

Figure 2 presents naloxone distribution rate trends based on the IQVIA and Symphony Health data. There is a notable increase in naloxone pharmacy distribution starting in 2016, which corresponds with expansion in state laws allowing naloxone distribution without a patient-specific prescription, FDA approval of the first intranasal formulation of naloxone (Narcan), and rising opioid-related overdose deaths nationwide (Figure 1).

In the remainder of this section, we present estimates for how adoption of different types of NALs affected pharmacy distribution of naloxone, first presenting aggregate effects (Section 5.1) and effects by prescriber type (Section 5.2) using the IQVIA data, then assessing potential heterogeneous policy effects by patient characteristics (Section 5.3) using the Symphony Health data.

5.1 Aggregate Results

Figure 3 presents event study results for the effects of our two NAL dimensions on total naloxone pharmacy distribution per capita using the IQVIA data (see Figure B2 for results using the Symphony Health data). We observe little evidence of pre-existing trends for either NAL dimension. The pre-period estimates are small and never statistically different from zero. Once adopted, we see an increase in the number of naloxone prescriptions dispensed for both NAL dimensions; however, the growth is substantially faster for prescriptive authority. In the first year after adoption, NPS prescription laws increase quarterly pharmacy naloxone purchases by 3.1 claims per 100,000; prescriptive authority laws increase purchases by 14.5 claims per 100,000. Converting these to annual rates implies an additional 46 claims per 100,000 population in prescriptive authority states relative to NPS prescription law states in the first year after law adoption. On average in the post-implementation period, we estimate significant increases of 3.3 quarterly claims per 100,000 for NPS prescription laws and 10.1 for prescriptive authority (see Table 2). This latter estimate represents a 75 percent increase in naloxone distribution in pharmacist prescriptive authority states over the estimated counterfactual. In Table 2, we also present results from tests of the null hypothesis that the two estimates are equal. We reject this hypothesis for the full sample, implying that prescriptive authority NALs improve naloxone pharmacy distribution more than NPS prescription laws.

5.2 Effects by prescriber type

While this evidence suggests that laws granting pharmacists prescriptive authority for naloxone have large effects in improving naloxone availability, there is little systematic evidence about how much naloxone is actually prescribed by pharmacists. Figure 4 shows the national time trend in quarterly naloxone claims prescribed by pharmacists, scaled by population (solid line) and by total naloxone claims (dashed line). We observe small increases beginning in 2016, followed by a sharp rise in 2018. In 2018, there were 5.7 naloxone claims per 100,000 prescribed by pharmacists. Despite this escalation, pharmacist-prescribed claims nationally compose a small share of all naloxone pharmacy claims – only 3.7 percent in 2018.

Restricting to states that adopted pharmacist prescriptive authority laws during our study period, Panel B shows the number of naloxone claims, as well as the share of all naloxone claims, prescribed by pharmacists over time relative to adoption of pharmacist prescriptive authority. We find that naloxone claims prescribed by pharmacists increase over time, both in terms of overall levels and the share of total naloxone claims. However, they still remain a small share of claims in these states. Notably, the increases observed in Panel B are too small to explain the large increases observed in Figure 2. We formalize this point in the next section.

In Figure 5, we study naloxone pharmacy fills, stratifying the analyses by transactions with a pharmacist or non-pharmacist listed as the prescriber of record. Panel A shows results when we only examine naloxone prescribed by pharmacists, while Panel B shows results when pharmacist prescribers are excluded. Panel A shows directly that pharmacist prescriptive authority laws indeed significantly increase naloxone prescribing by pharmacists, consistent with the purported causal mechanism of action of these laws. While the absolute magnitude of the increase in pharmacist-prescribed naloxone in pharmacist prescriptive authority states is

relatively small, it represents a 180 percent increase over the estimated counterfactual rate (Table 2).[9] Interestingly, we also estimate large and statistically significant increases resulting from adoption of pharmacist prescriptive authority in Panel B, where naloxone fills prescribed by pharmacists are excluded. Estimates imply a 95 percent increase in naloxone fills from non-pharmacist prescribers following pharmacist prescriptive authority laws, effects which are comparable in relative terms to the overall effects shown in Figure 2.

These results provide strong evidence of spillovers associated with pharmacist prescriptive authority laws. While the laws have larger relative effects on increasing naloxone distribution by pharmacist versus non-pharmacist prescribers, pharmacists remain responsible for a relatively small (although increasing; see Figure 4) proportion of naloxone dispensed. Thus, while pharmacists are increasingly likely to prescribe naloxone after prescriptive authority law adoption, the majority of the increase in naloxone found in pharmacist prescriptive authority states stems from pharmacists increasingly dispensing naloxone prescribed by other providers (including potentially via NPS prescription), rather than their own prescribing.

Figure 6 disaggregates these spillovers for different types of prescribers. We stratified the analysis for the five most common naloxone prescriber types in the data plus a separate category that aggregates all other prescribers (excluding pharmacists). In all cases, we see evidence of

[9] We also find very small but significant increases in pharmacist-prescribed naloxone in NPS prescription law states (Table 2), prompting questions regarding how pharmacists can be listed as prescribers in state-quarters when they do not have pharmacist prescriptive authority. As only one provider type is allowed to be indicated on IQVIA records, this could reflect individuals who have pharmacy degrees but can also prescribe in a separate capacity (e.g., a PharmD who is also a nurse practitioner). However, it may also indicate errors in entry of prescriber of record or errors in IQVIA's linkage of prescription to provider type. Excluding states that ever authorized pharmacist prescriptive authority, fewer than 1 percent of naloxone fills have a pharmacist listed as prescriber, and those that do often have a standing or protocol order policy that specifically indicates the pharmacist should be listed as the prescriber of record (e.g., Kansas; see Figure A1).

large increases. These increases are generally larger in prescriptive authority states relative to NPS prescription law states, with the notable exception of pain medicine prescribers (see Table 2). The largest absolute increases are observed among family medicine physicians and nurse practitioners. Interestingly, among non-pharmacist providers, prescriptive authority NALs improve naloxone pharmacy access significantly more than NPS prescription laws only for physician assistants and family medicine physicians. Prescription fills from internal medicine providers show very similar patterns following the adoption of NPS prescription laws and pharmacist prescriptive authority laws, while only NPS prescription laws show significant effects in increasing naloxone claims from pain medicine prescribers.

5.3 Effects by patient characteristics

Race/Ethnicity

We present our event study results by race/ethnicity in Figure 7; the corresponding difference-in-differences estimates are provided in Table 3. While pharmacist prescriptive authority laws increase claims more than NPS prescription laws for each population, this difference is statistically significant only for non-Hispanic Black individuals. We estimate that NPS prescription laws increase quarterly naloxone purchases by 2.3 claims per 100,000 for non-Hispanic White individuals; prescriptive authority increases quarterly claims by 4.2 per 100,000, implying a 118% increase from the counterfactual. Effects for non-Hispanic Whites in NPS prescription law and prescriptive authority states do not significantly differ from each other.

The prescriptive authority effect is largest for non-Hispanic Black individuals at 6.5 per 100,000. This estimate implies that these policies increase naloxone pharmacy claims tenfold. The point estimates and implied proportional effects are smaller for Hispanics and for the

"Other" race/ethnicity category, and they are more imprecisely estimated. These results suggest that the high rate of naloxone claims for non-Hispanic Black individuals is driven by NALs. Without NALs, non-Hispanic Whites would have had higher naloxone purchasing rates than non-Hispanic Black individuals in 2017. Instead, we observe the opposite ordering. The high rate of naloxone claims for non-Hispanic Black individuals appears to be due to their responsiveness to naloxone access policies.

Sex

Figure 8 presents effect estimates separately for men and women. For men, NPS prescription laws increase quarterly naloxone claims by 2.4 per 100,000 while prescriptive authority policies increase claims by 7.3 per 100,000, a difference that is statistically significant at the 10 percent level (see Table 3). The prescriptive authority estimate implies a 140 percent increase from baseline. Women experience similar gains, with relatively larger increases following adoption of pharmacist prescriptive authority; NPS prescription laws increase naloxone purchases by 2.9 per 100,000 women and prescriptive authority increases claims by 7.3 per 100,000 women. The difference between the two policies is statistically different at the 5 percent level, and the prescriptive authority effect size suggests a 122 percent increase from the implied counterfactual. These estimates imply that women would purchase more naloxone than men even without NALs. However, NALs have substantially increased the sex naloxone gap.

Age Groups

Figure 9 shows event study estimates by age group. We observe positive effects for both types of NAL across all age groups. The 0–18 age group experienced small effects in absolute terms; however, these represent large relative effect sizes (85 percent and 129 percent increases

for NPS prescription and pharmacist prescriptive authority laws, respectively) due to their low baseline rates. For pharmacist prescriptive authority laws, the effect sizes are largest for the 19—38 and 39–58 age groups (155 percent and 147 percent increases, respectively); for both age groups, pharmacist prescriptive authority laws increase naloxone fills by between 9 to 11 per 100,000, effects that are statistically larger than the effect produced by NPS prescription laws (see Table 3). The effect sizes for the oldest age group implies an 89 percent increase for pharmacist prescriptive authority and 31 percent increase for NPS prescription laws, effects which do not statistically differ from each other.

5. Sensitivity Analyses

We test the sensitivity of the main results to some of our modeling assumptions. First, we provide evidence that the exclusion of other types of NALs (i.e., state NALs that do not authorize NPS prescription distribution or provide prescriptive authority to pharmacists; hereafter, "weak" NALs) is not driving our results. Figure A3 presents event study estimates of the effects of "weak" NALs on naloxone pharmacy distribution based on the IQVIA data.[10] We observe evidence of a small, statistically insignificant increase upon adoption, the size of which is *substantially* smaller than those estimated for NPS prescription laws or pharmacist prescriptive authority. This suggests that our previous results are not driven by omission of the weaker NALs.

Next, we consider the role of the state- and time-varying covariates. Figure A4 presents event study estimates which do not adjust for these other factors. Figure A5 adds additional predictors to address concerns that illicitly manufactured fentanyl, which became increasingly

[10] We exclude states that had adopted these laws prior to 2010 (since we do not observe an untreated period for them) and we limit the sample to 2010-June 2017 since every state had adopted a NAL by the second half of 2017.

incorporated into the U.S. drug supply around the same time our policies of interest were increasingly adopted, may independently drive purchasing of naloxone. To model the fentanyl crisis, we add a control for non-medical OxyContin misuse rates (measured in 2004–2009 prior to reformulation) interacted with year indicators; these interactions have been shown to predict a large share of heroin (Alpert et al., 2018) and synthetic opioid deaths (Powell & Pacula, 2021).[11] Across both sets of analyses, our estimates are relatively unaffected by the exclusion or inclusion of these controls. Our results are also relatively unchanged in models that do not weight by population (Figure A6).

We also test the appropriateness of using a pre-period dating back to 2010. The opioid crisis changed substantially throughout this time period so one concern is that the pre-period may not be reflective of counterfactual state-level conditions (in the absence of treatment) in the post-period, especially since our 2sDID approach estimates the baseline parameters in the pre-period. A related concern is that naloxone distribution rates were very low in the early years of our sample period and may not represent appropriate levels to estimate underlying state-level heterogeneity. To test the importance of these concerns, we shorten our sample to begin in 2014, exploiting only policies which were adopted during this short window. This selection makes six states always-treated so we must exclude these (California, Kentucky, New Jersey, North Carolina, Oregon, and Vermont) from the analysis. Figure A7 shows that effects are similar over this shortened timeframe, suggesting results are insensitive to choice of pre-period length.

Finally, estimating the effects of NPS prescription laws using imputation-based methods necessitates truncating the sample to time periods in which there were non-adopters in order to

[11] While these variables may not fully predict the geography of the fentanyl crisis, Powell & Pacula (2021) show that setting the 2017 interaction to zero would eliminate all excess (relative to 2010) synthetic opioid deaths in 2017.

identify the time fixed effects. However, we can still compare the effects of pharmacist prescriptive authority laws relative to NPS prescription laws in later periods (i.e., including all the 2017 and 2018 data). We thus implement a complementary analysis which uses NPS prescription law observations as control units to estimate the relative effects of prescriptive authority laws. Because both types of laws show dynamic effects post implementation, we match each prescriptive authority state with NPS prescription states adopting in the same quarter. This holds constant the calendar time and time-relative-to-adoption between both types of policies; by including time fixed effects that vary by "cohort" (when the law was effective), each prescriptive authority state is only compared to states adopting NPS prescription laws at the same time. We use the same estimation approach as before but include these cohort*quarter interactions.

Results, presented in Figure A8, are consistent with our main findings that pharmacist prescriptive authority laws increase naloxone claim rates significantly more than NPS prescription laws.

Figures B3–B8 show analogous results from all previously described analyses using the Symphony Health data, all of which are consistent with the aforementioned findings. We observe similar patterns across the two data sets.

6. Conclusion

In 2021, an estimated 107,000 Americans died from a drug overdose, continuing the two decades long upward trend despite substantial government efforts to change the trajectory (Spencer et al., 2022). The toll of opioid-related mortality is high enough to bear responsibility for declining U.S. life expectancy after 2013 (Currie & Schwandt, 2020). While there have been dramatic increases in the volume of naloxone dispensed from pharmacies, there remains

substantial need to further expand access through both community-based programs and pharmacy channels (Irvine et al., 2022).

Our findings suggest that laws removing the need for an individual to obtain a patient-specific prescription prior to entering a pharmacy are a highly effective policy lever for expanding naloxone distribution through pharmacies. Both NPS prescription and pharmacist prescriptive authority laws significantly increase naloxone distribution through this channel. However, the effects of pharmacist prescriptive authority laws appear to be significantly larger than those of NPS prescription laws, and their effects manifest more rapidly. From our analyses by prescriber type, we learn that the relatively larger effects of pharmacist prescriptive authority laws do not derive entirely from changes in pharmacist behavior. Rather, we observe considerable increases in naloxone distribution across a variety of prescriber types following adoption of pharmacist prescriptive authority laws, and in most instances these responses are even larger than other those seen after adoption of non-patient specific distribution policies. This suggests that pharmacist prescriptive authority laws may have substantial spillovers, increasing naloxone prescribing beyond those pharmacists directly targeted by the policy.

Spillovers in the health care context are common in numerous contexts (Chandra & Staiger, 2007; Hodor, 2021; Pauly & Pagán, 2007), including spillovers related to improving pharmaceutical access (Alpert et al., 2015). By reducing the barriers to obtaining naloxone through some channels, prescriptive authority laws may potentially increase prescribing of naloxone more generally. It is unclear precisely why this is the case. The laws themselves may have amplified the salience of the policy to improve naloxone access, altering demand and prescriber behavior. It is also possible that passage of these laws incentivized pharmacies to stock naloxone given that they could directly prescribe the naloxone themselves. Naloxone

stocking in pharmacies is often an issue (Abbas et al., 2021; Eldridge et al., 2020; Graves et al., 2019; Meyerson et al., 2018; Spivey et al., 2020), and prescriptive authority policies may better incentivize pharmacy stocking, further reducing the hassle of obtaining naloxone for those who already have a prescription and encouraging additional prescribing. Because we only observe filled prescriptions, we cannot directly assess the extent to which individuals might be unable to fill naloxone prescriptions due to inadequate pharmacy supply.

While we are unable to ascertain why prescriptive authority laws increase prescribing of naloxone by non-pharmacist providers, we are able to consider which patient population groups benefit the most by these policies. While we find increases in prescriptions filled by patients from all racial/ethnic, sex, and age groups in response to both NPS prescription laws and pharmacist prescriptive authority, we see that that Non-White groups, particularly Non-Hispanic Black individuals, benefit even more from these prescriptive authority policies than from NPS prescription laws. Both men and women experience greater naloxone fills under prescriptive authority laws, and we see significantly larger impacts of these laws for patients between the ages of 39–58 and 59+. These findings suggest that, while pharmacies remain a relatively small share of total naloxone dispensed, these policies are effective at increasing access to naloxone for specific demographic groups that are particularly vulnerable to rising overdose deaths from fentanyl (Kariisa et al., 2022). They also suggest that the recent FDA policy to make Narcan available over-the-counter (OTC) may do even more to provide greater access to this drug, provided the price of the OTC remains affordable.

Our results are subject to several limitations. First, we use the date in which the state law went into effect, not the date of implementation, or when these policies became widely known by pharmacists and other prescribers. Our event study analyses help address the problem of

implementation lags by considering delayed effects of these policies, but the use of methods robust to heterogenous policy effects across states and over time comes with the tradeoff of truncating our data series such that we cannot reliably estimate longer term effects past 2018.[12] Second, we only examine the impact of these policies through one access channel for naloxone: pharmacies. While our limited focus on pharmacies is helpful for identifying the direct impact of the policy on the targeted mechanism, it is possible that these policies—particularly NPS prescription laws—have additional impacts through other channels. Additional work should consider the possible impact these policies have on distribution of naloxone through overdose education and naloxone distribution programs, law enforcement, and in criminal justice settings, which are not necessarily captured through pharmacy data. Third, while we are able to examine the role of pharmacist prescribers in prescriptive authority states, we cannot test the analogous mechanism in NPS prescription states because prescriptions dispensed via standing or protocol orders are not identifiable in our datasets.

The appeal of improving naloxone access as a policy option stems in part from the limitations that have been exposed in alternative types of methods to curb opioid-related harms. Supply-side interventions are intended to limit access to opioids in which misuse propensities are high by reducing prescription length (Sacks et al., 2021), requiring monitoring of patient prescribing histories (Alpert et al., 2020; Buchmueller & Carey, 2018), or removing more easily abusable versions of an opioid drug (Alpert et al., 2018). Unfortunately, these interventions can potentially disrupt legitimate opioid use and even incentivize patients to enter illicit markets to

[12] With homogenous treatment effects and estimating a TWFE model, the specification implicitly assumes that any effect estimated among early adopters holds in later periods, permitting estimation of relative differences for a second policy. With heterogeneous treatment effects, this type of extrapolation is not imposed.

obtain opioids non-medically (Kim, 2021; Mallatt, 2022; Meinhofer, 2018; Powell & Pacula, 2021). Demand-side interventions, such as improving treatment access, rely on people with opioid use disorder seeking treatment, which surveys suggest is unlikely for more than half of those in need (Han et al., 2017). Harm reductions interventions, such as increased naloxone access, do not solve the underlying problem, but they protect individuals from experiencing worse, sometimes fatal, consequences and can, at times, motivate individuals to seek treatment who would otherwise be unwilling.

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FIGURES

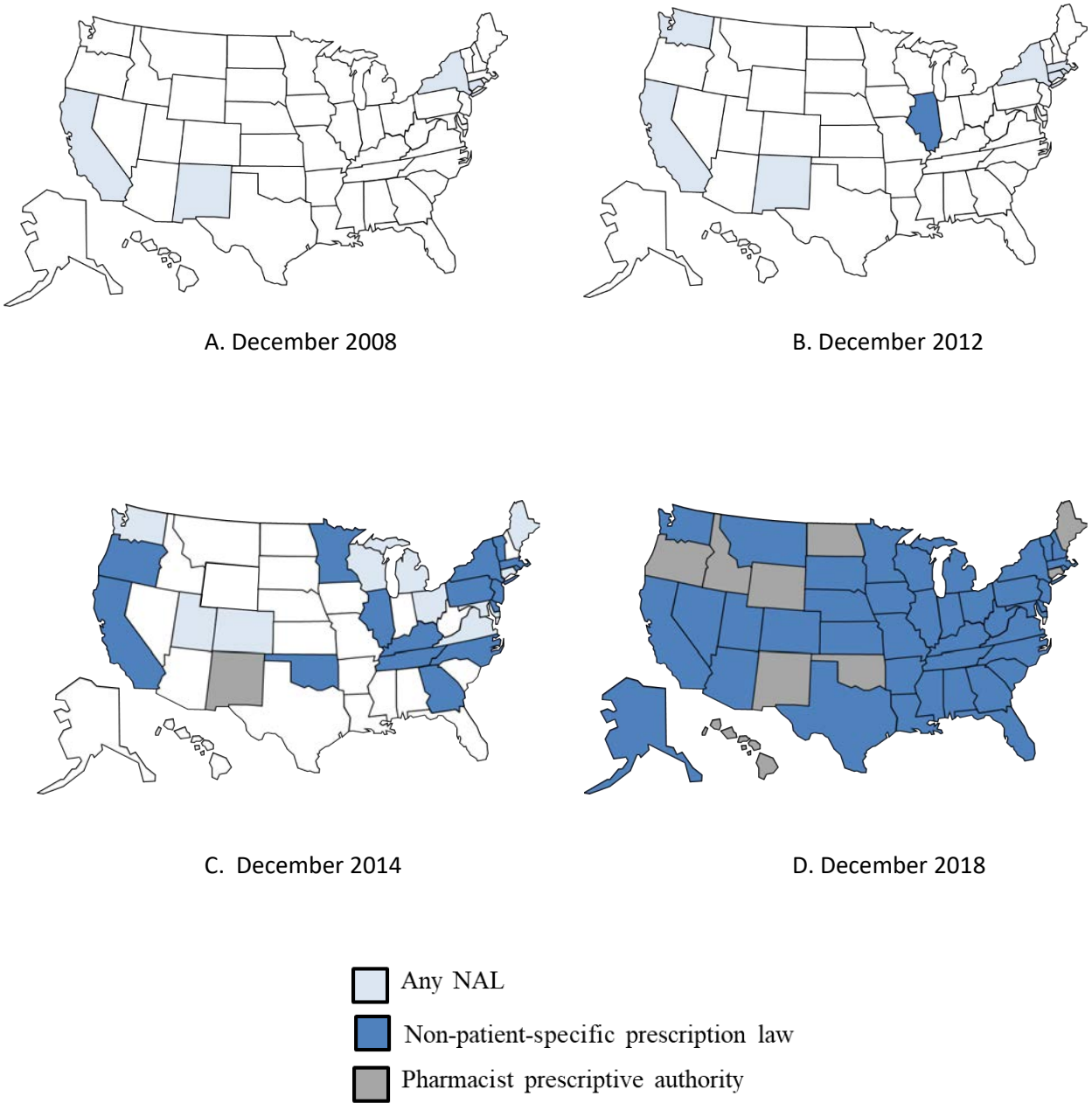


Figure 1: Map of State Naloxone Access Laws Over Time

Notes and sources: PDAPS and authors' own legal analysis. As of December 2018, the following states had both non-patient-specific prescription and pharmacist prescriptive authority laws: Connecticut, District of Columbia, Hawaii, Maine, New Mexico, Oklahoma, Oregon, and Wyoming.

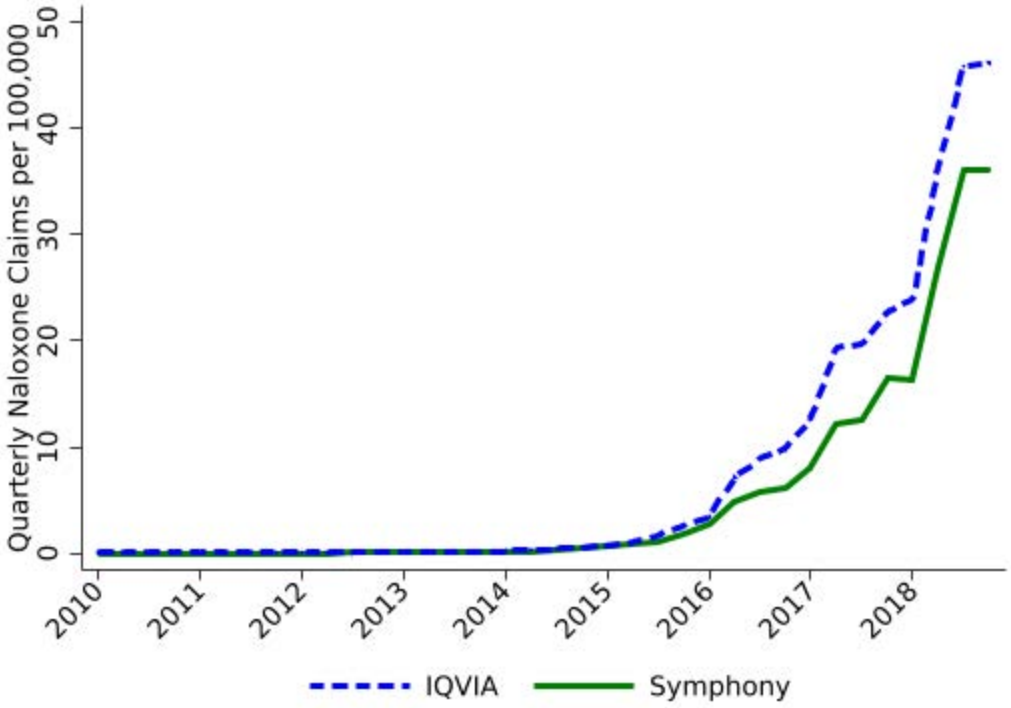


Figure 2: Quarterly National Naloxone Claims per 100,000

Notes and sources: IQVIA and Symphony Health Data

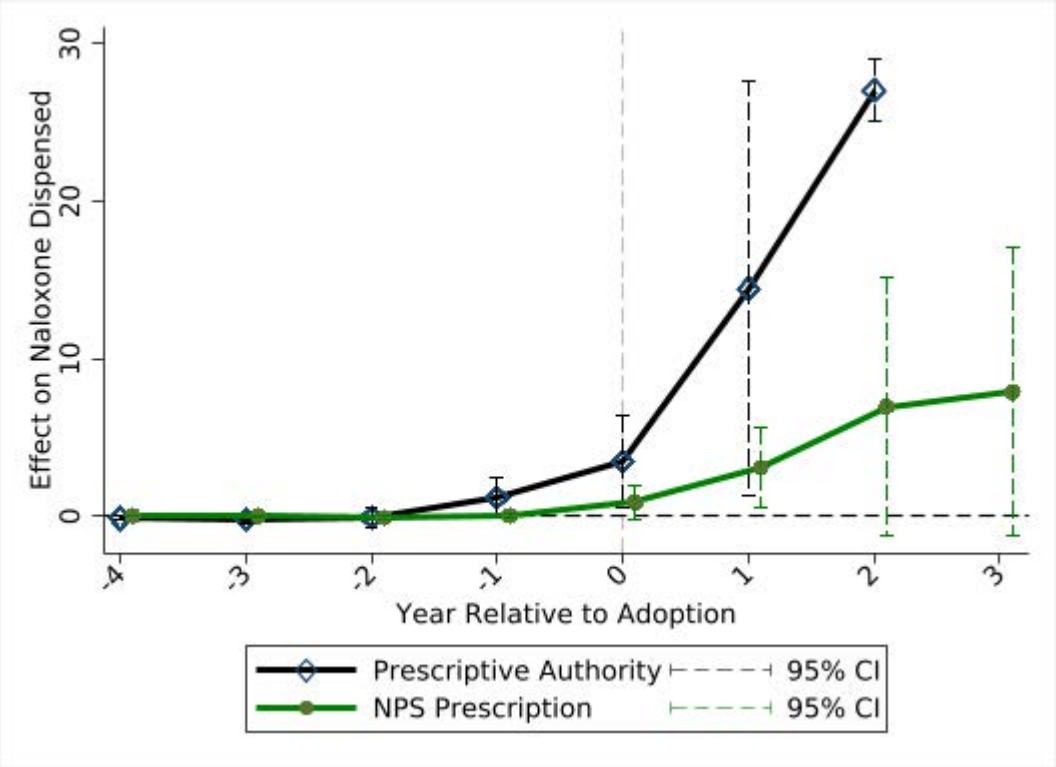
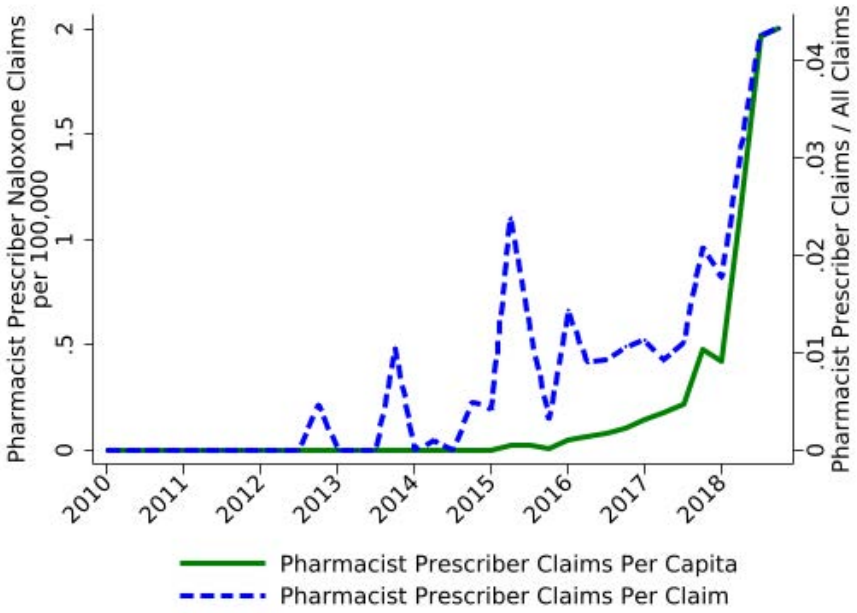
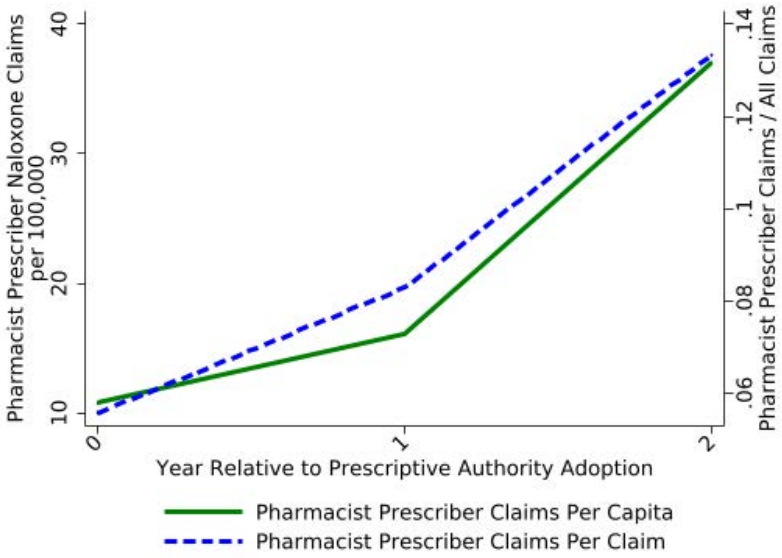


Figure 3: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates

Notes and sources: IQVIA data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no non-patient-specific prescription or pharmacist prescriptive authority laws) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.



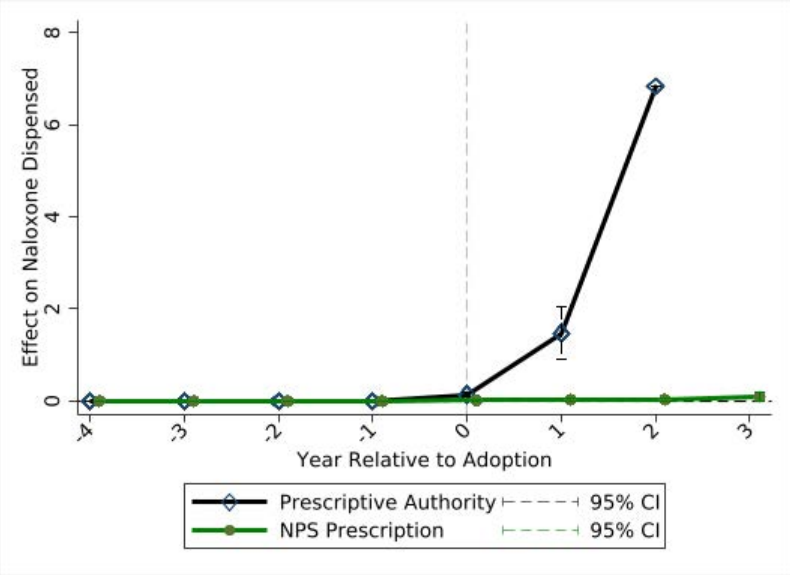
A. Pharmacist-Prescribed Naloxone Claims by Quarter



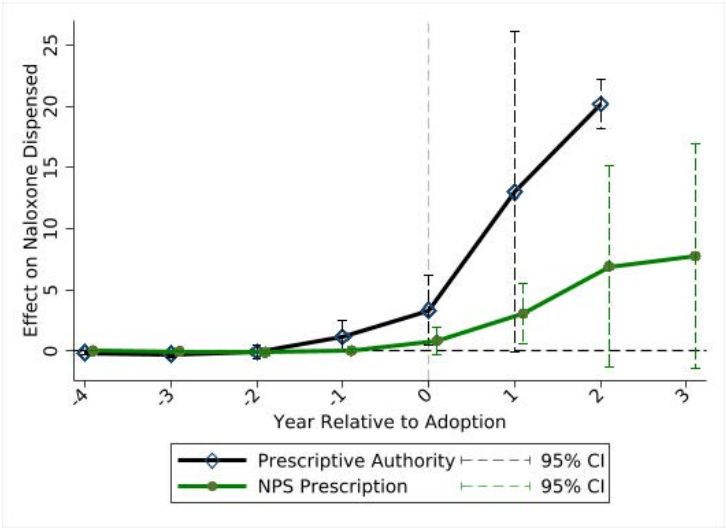
B. Pharmacist-Prescribed Naloxone Claims in Pharmacist Prescriptive Authority States, By Year Relative to Adoption

Figure 4: Quarterly Pharmacist-Prescribed Naloxone Claims per 100,000

Notes and sources: IQVIA data. Panel A includes all states. Panel B only includes states with active pharmacist prescriptive authority policies. Panel B aggregates to the annual level.



A: Pharmacist-prescribed claims only



B: Excluding pharmacist-prescribed claims

Figure 5: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, by Pharmacist or Non-Pharmacist

Notes and sources: IQVIA data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

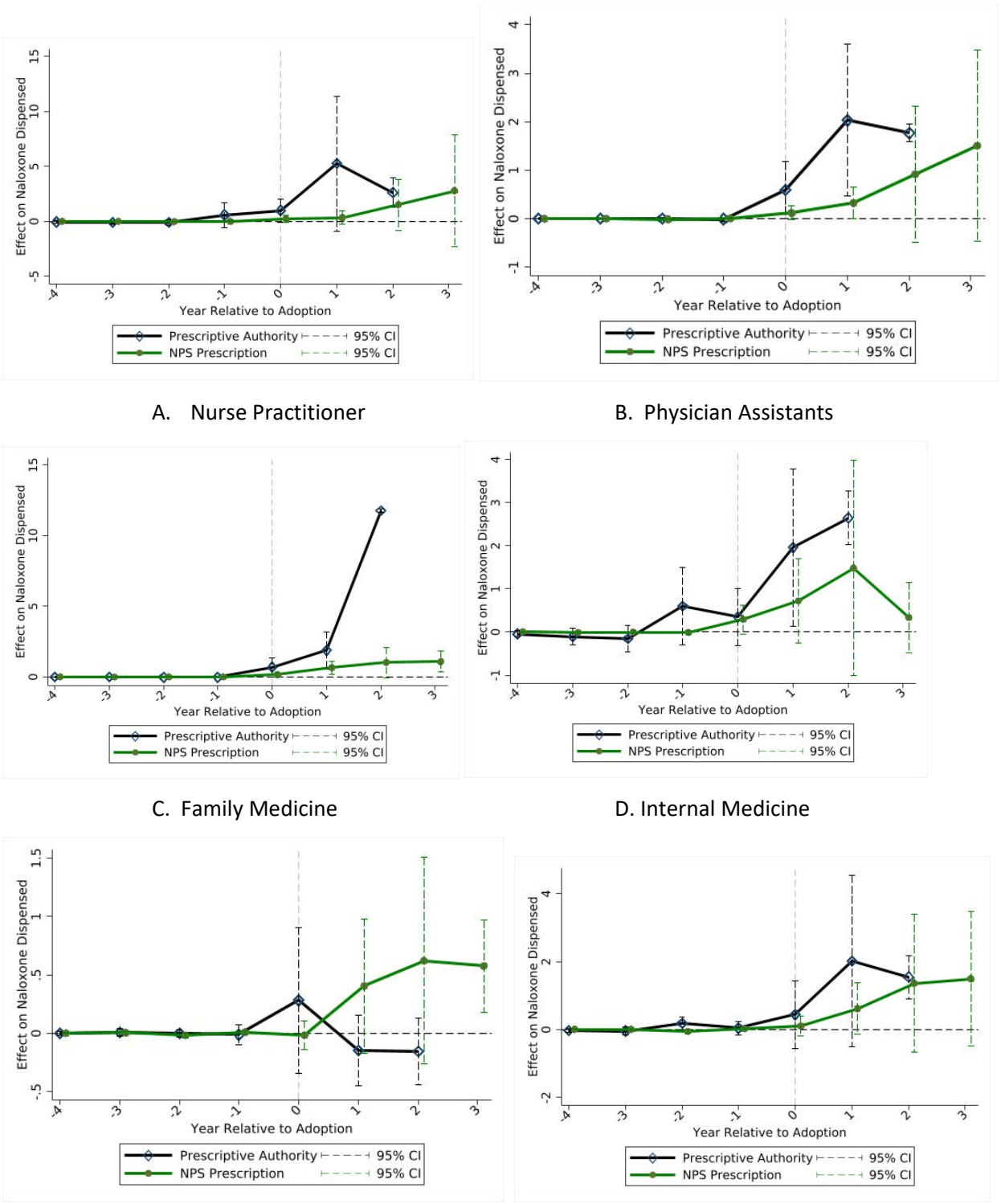
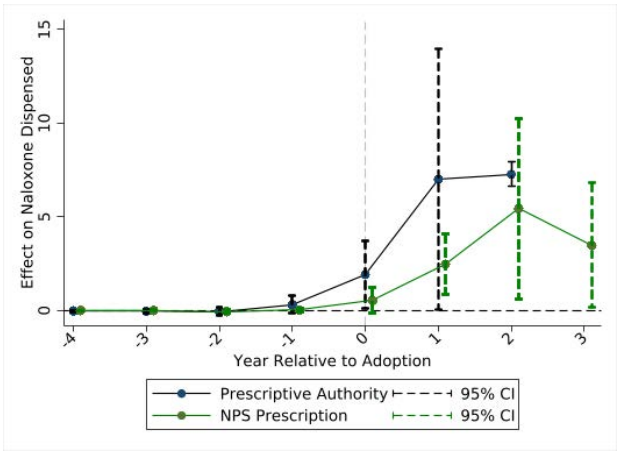
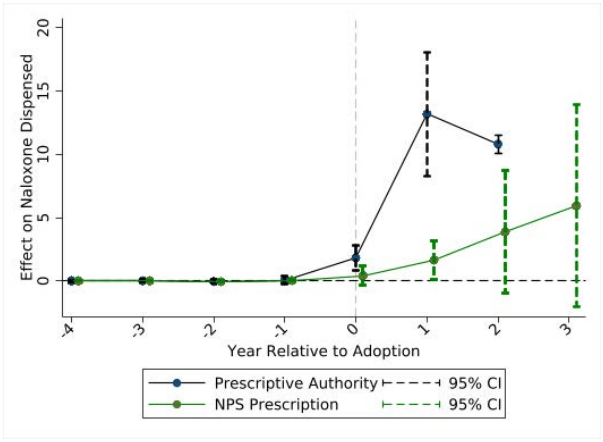


Figure 6: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, by Prescriber Type

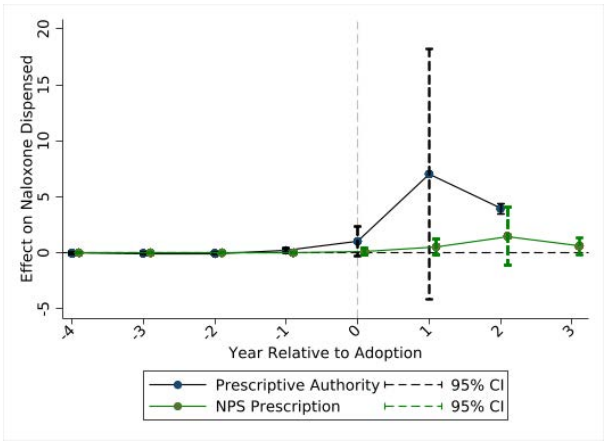
Notes and sources: See Figure 3. All outcomes defined by source of prescription. "Other" excludes pharmacists and the categories listed in Panels A-E.



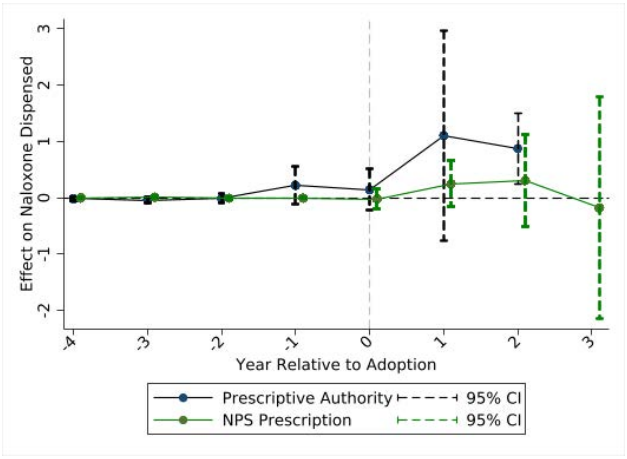
A. Non-Hispanic White



B. Non-Hispanic Black



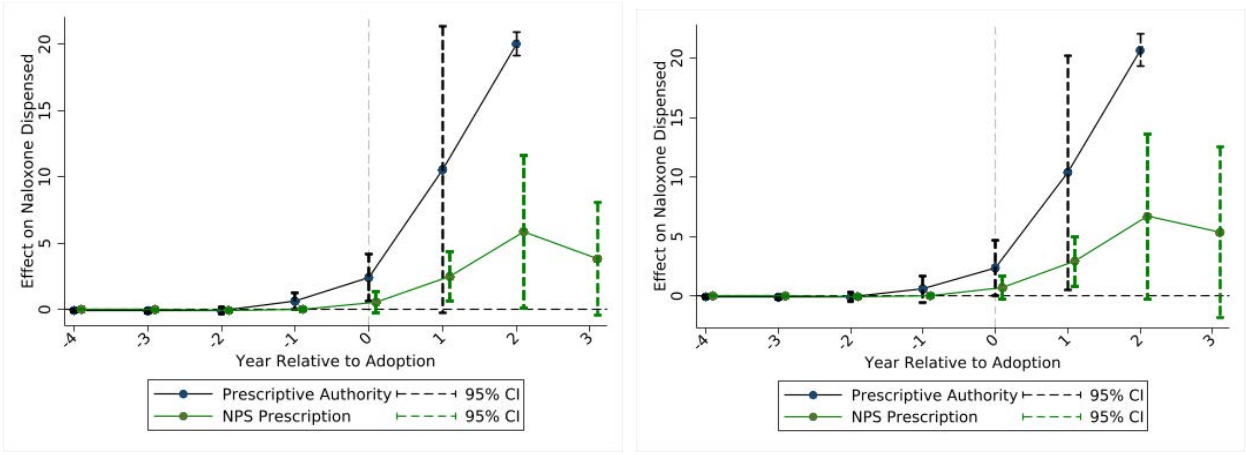
C. Hispanic



D. Other

Figure 7: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, by Race/Ethnicity

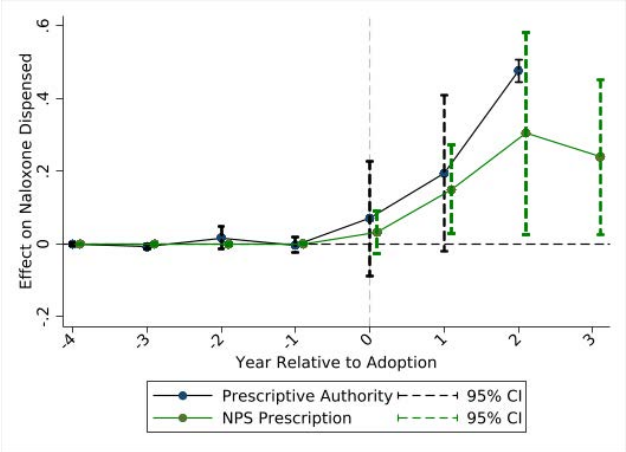
Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.



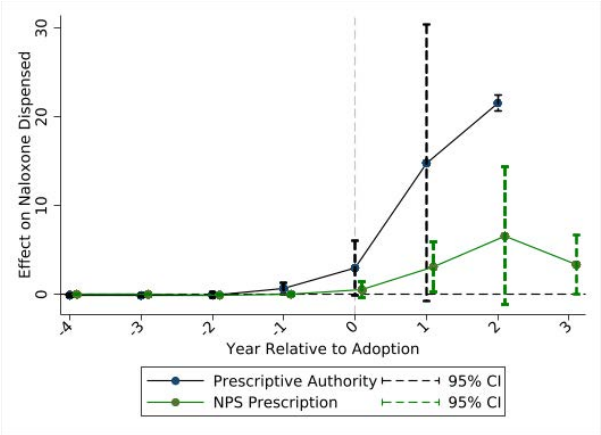
A. Male B. Female

Figure 8: Event Study Estimates, by Sex

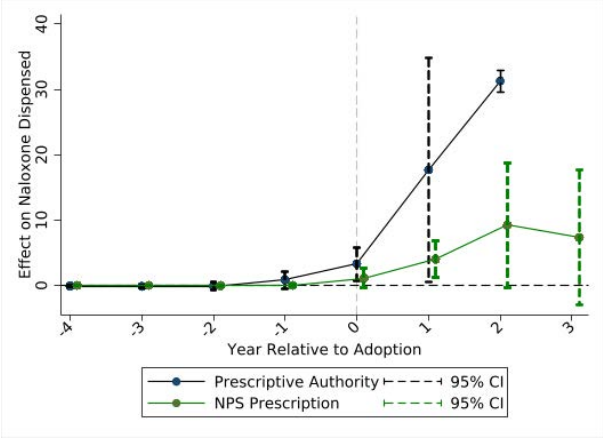
Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.



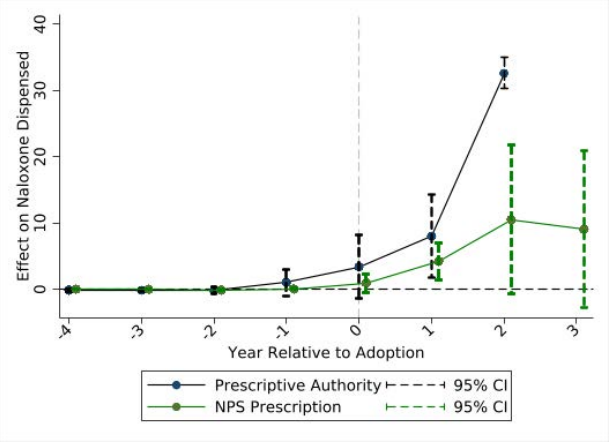
A. Ages 0-18



B. Ages 19-38



C. Ages 39-58



D. Ages 59+

Figure 9: Event Study Estimates, by Age Group

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

TABLES

Table 1: State Naloxone Access Laws (NALs) as of December 2018

	Any NAL	Non-patient-specific prescription law	Pharmacist prescriptive authority law
Alabama	July 2015	July 2015	
Alaska	April 2016	April 2016	
Arizona	September 2016	September 2016	
Arkansas	August 2015	August 2015	
California	January 2008	January 2014	
Colorado	June 2013	May 2015	
Connecticut	October 2003	October 2017	July 2015
Delaware	September 2014	September 2014	
District of Columbia	April 2013	March 2017	March 2017
Florida	July 2015	July 2016	
Georgia	May 2014	May 2014	
Hawaii	July 2016	July 2016	July 2018[a]
Idaho	July 2015		July 2015
Illinois	January 2010[a]	January 2010[b]	
Indiana	May 2015	May 2015	
Iowa	June 2016[b]	June 2016 [c]	
Kansas	July 2017	July 2017	
Kentucky	July 2013	July 2013	
Louisiana	September 2015	September 2015	
Maine	May 2014	November 2015	June 2018[a]
Maryland	October 2013	October 2015	
Massachusetts	September 2012	July 2014	
Michigan	November 2014	April 2017	
Minnesota	June 2014	June 2014	
Mississippi	July 2015	July 2015	
Missouri	September 2016	September 2016	
Montana	May 2017	May 2017	
Nebraska	June 2015	September 2018	
Nevada	October 2015	October 2015	
New Hampshire	July 2015	July 2015	
New Jersey	July 2013	July 2013	
New Mexico	May 2001	April 2016	April 2014
New York	Apr 2006	July 2014	
North Carolina	May 2013	May 2013	
North Dakota	August 2015	August 2015	April 2016
Ohio	April 2014	August 2015	

Oklahoma	November 2013	November 2014	November 2017[a]
Oregon	July 2013	July 2013	April 2016
Pennsylvania	December 2014	December 2014	
Rhode Island	July 2012	April 2014	
South Carolina	July 2015	July 2016	
South Dakota	July 2016	July 2016	
Tennessee	July 2014	July 2014	
Texas	September 2015	September 2015	
Utah	June 2014	June 2016	
Vermont	July 2013	July 2013	
Virginia	July 2013	May 2015	
Washington	July 2010	August 2015	
West Virginia	June 2015	July 2016	
Wisconsin	May 2014	January 2016	
Wyoming	July 2017	July 2017	July 2017

Notes: Effective month is assigned based on the first full month the law was effective. I.e., if the law was effective the first of a given month, that month is used; if the law was effective the 2nd or later in a given month, the subsequent month is used.

[a]To ensure sufficient control units, our primary analysis truncates the sample period to end in 2017q2, and thus these states are not used to identify pharmacist prescriptive authority law effects except in a sensitivity analysis.

[b]Because Illinois had a standing order before the beginning of our sample period, we exclude it from our policy effect analyses. The imputation approach requires estimates of a state fixed effect, which is not possible if we never observe the state as untreated. However, we include Illinois when presenting summary statistics.

[c]Note that for Iowa, the legislature adopted two different bills (one house bill and one senate bill) regarding the naloxone access law section, both with an effective date of May 27, 2016. However, one amended the section and made those amendments retroactive to April 6, 2016, implying that the entire statute must be retroactive to April 6, 2016; for the purposes of our analyses, however, we use the May date.

Table 2: Difference-in-Differences Estimates for Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority Laws on Pharmacy-Based Naloxone Distribution Rates, by Prescriber Type

	Non-patient-specific prescription (NPS Rx)	Pharmacist prescriptive authority (Rx Auth)	p-value for NPS Rx law = Rx Auth	Counterfactual outcome (NPS Rx)	Counterfactual outcome (Rx Auth)
All	3.283** (1.292)	10.056*** (2.761)	0.022**	13.416	9.873
Pharmacist prescribers	0.028** (0.012)	1.360** (0.664)	0.045**	0.057	0.759
Excluding pharmacists	3.254** (1.293)	8.696*** (2.678)	0.060*	13.359	9.115
By non-pharmacist prescriber type:					
Nurse practitioners	0.692 (0.422)	2.714** (1.359)	0.142	2.667	1.961
Physician assistants	0.439** (0.207)	1.240*** (0.308)	0.029**	1.342	1.016
Family medicine	0.550*** (0.170)	2.342** (0.803)	0.029**	1.850	2.118
Internal medicine	0.667 (0.438)	1.186** (0.495)	0.406	1.959	1.495
Pain medicine	0.288* (0.166)	0.079 (0.248)	0.423	1.548	0.441
All others	0.618* (0.331)	1.136 (0.750)	0.512	3.993	2.084

Notes: ***1%, **5%, *10% statistical significance. The outcome is quarterly naloxone claims per 100,000 based on IQVIA data. Standard errors in parentheses adjusted for state-level clustering and two-step estimation process. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. All models and estimates are population-weighted. P-value in brackets is result of a test of equality of the NPS prescription and pharmacist prescriptive authority law estimates. Counterfactual outcome (Rx Auth) is the estimated number of claims per 100,000 in prescriptive authority state-quarters if they had no prescriptive authority (or a NPS prescription law). Counterfactual outcome (NPS Rx) is the 2017 number of claims per 100,000 in NPS prescription law state-quarters if they had no NPS prescription law.

Table 3: Difference-in-Differences Estimates for Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority Laws on Pharmacy-Based Naloxone Distribution Rates, No Covariates, by Patient Characteristics

	Non-patient-specific prescription (NPS Rx)	Pharmacist prescriptive authority (Rx Auth)	p-value for NPS Rx law = Rx Auth	Counterfactual outcome (NPS Rx)	Counterfactual outcome (Rx Auth)
All	2.617*** (0.947)	7.096*** (1.998)	0.040**	7.797	5.439
By race/ethnicity					
Non-Hispanic White	2.288*** (0.736)	4.157*** (1.608)	0.283	6.004	3.534
Non-Hispanic Black	1.856** (0.783)	6.469*** (1.822)	0.019**	6.022	0.661
Hispanic	0.552 (0.392)	3.776* (2.222)	0.155	1.773	3.092
Other	0.122 (0.227)	0.587 (0.397)	0.245	1.290	1.356
By sex					
Men	2.441*** (0.906)	7.302*** (2.337)	0.050*	7.175	5.223
Women	2.920** (1.064)	7.292*** (1.838)	0.035**	8.839	5.978
By age group					
0-18	0.138*** (0.053)	0.162** (0.076)	0.794	0.163	0.126
19-38	2.749** (1.291)	9.262** (3.606)	0.089*	7.059	5.962
39-58	4.075*** (1.475)	11.505*** (3.409)	0.042**	12.504	7.838
59+	4.441*** (1.555)	8.159*** (2.191)	0.153	14.330	9.151

Notes: ***1%, **5%, *10% statistical significance. The outcome is quarterly naloxone claims per 100,000 based on Symphony Health data. Standard errors in parentheses adjusted for state-level clustering and two-step estimation process. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. All models and estimates are population-weighted. P-value in brackets is result of a test of equality of the NPS prescription and pharmacist prescriptive authority law estimates. Counterfactual outcome (Rx Auth) is the estimated number of claims per 100,000 in prescriptive authority state-quarters if they had no prescriptive authority (or a NPS prescription law). Counterfactual outcome (NPS Rx) is the estimated 2017 number of claims per 100,000 in NPS prescription law state-quarters if they had no NPS prescription law.

APPENDIX A

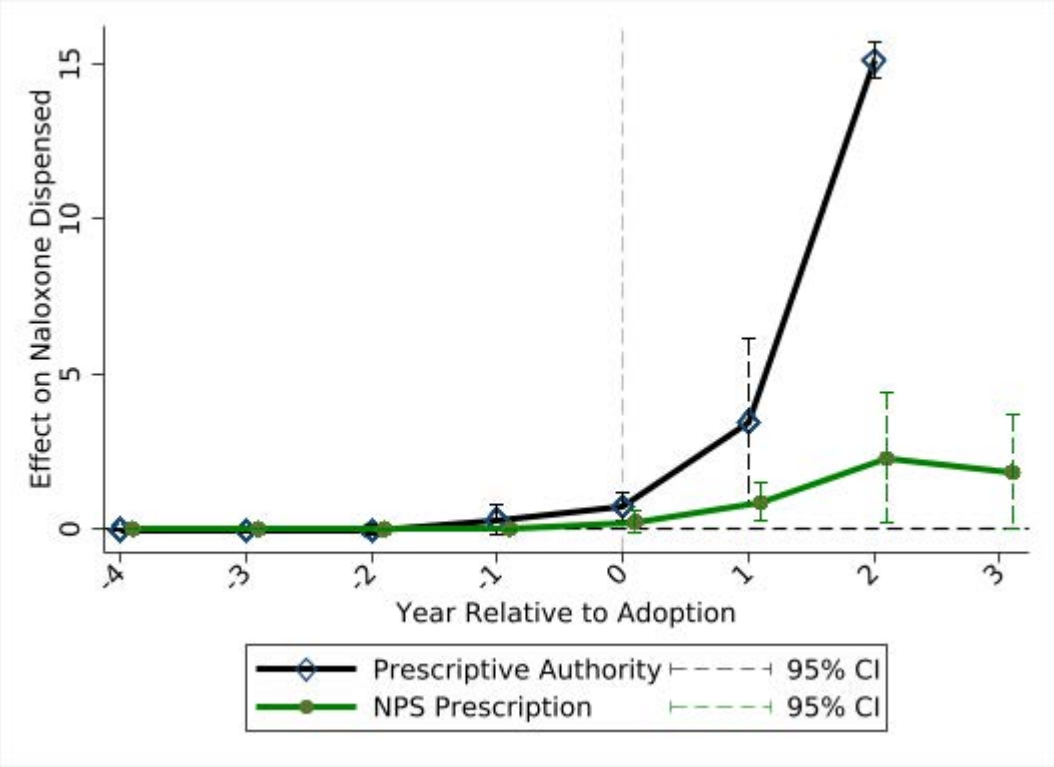


Figure A1: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Naloxone Claims with Missing Race/Ethnicity Data

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. The outcome is the number of naloxone claims per 100,000 in which race/ethnicity is unknown. We condition on state fixed effects, month fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no standing order or prescriptive authority) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

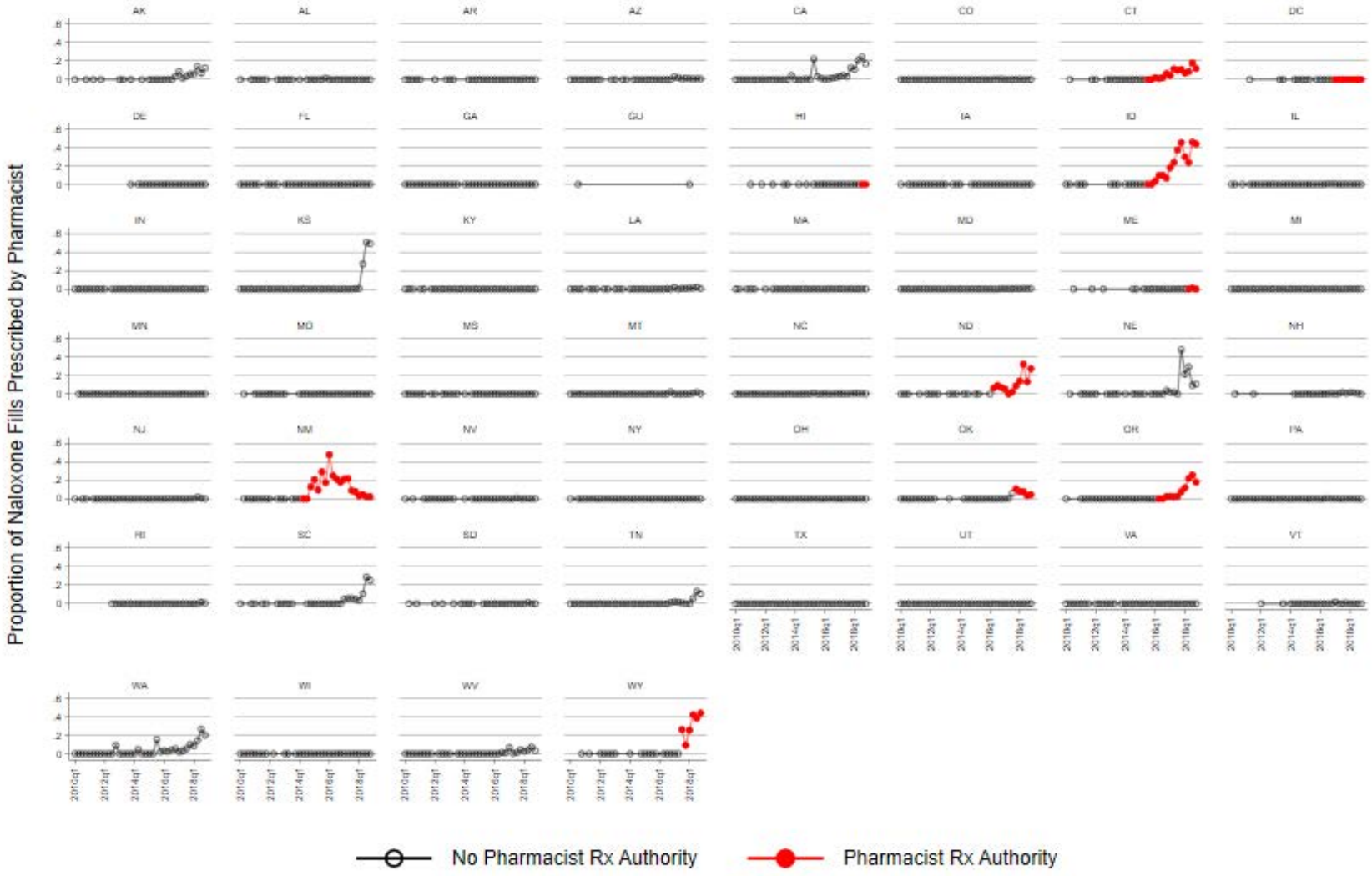


Figure A2: Proportion of Naloxone Fills Prescribed by a Pharmacist, by State and Pharmacist Prescriptive Authority Status

Notes and sources: IQVIA data.

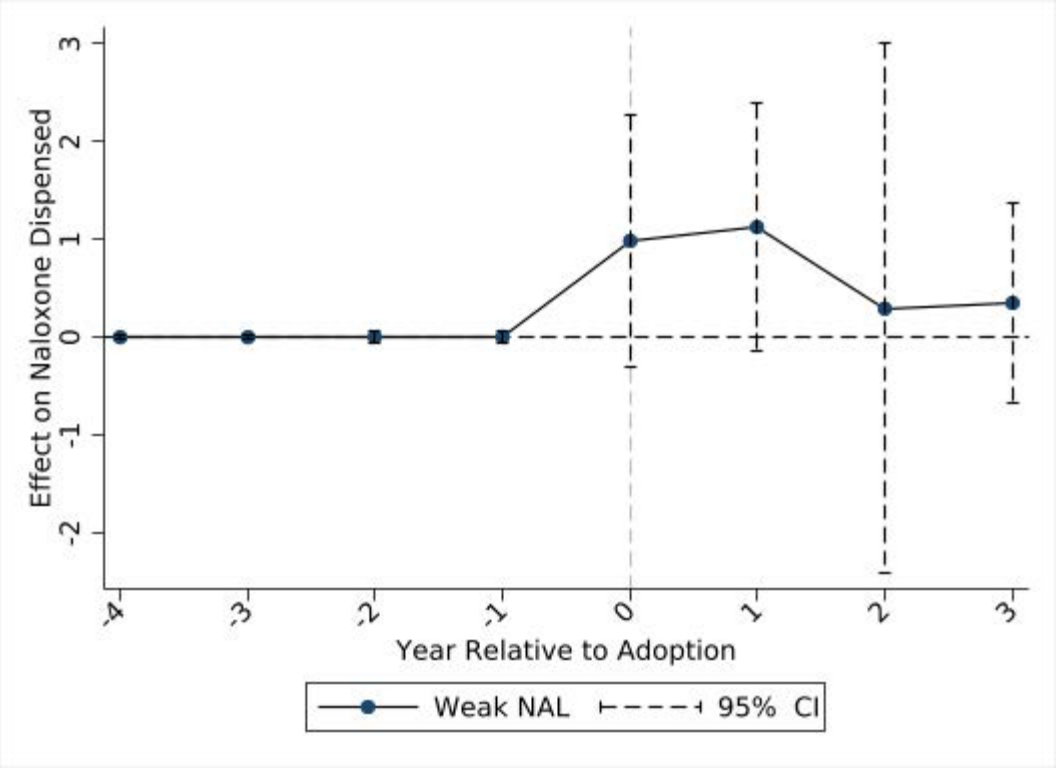


Figure A3: Effects of "Weak" NALs on Pharmacy-Based Naloxone Distribution Rates

Notes and sources: IQVIA data. N=1021. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. States are excluded once they adopt a non-patient-specific prescription law or pharmacist prescriptive authority law. States with NALs before 2010 are also excluded since they are always-treated. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. All models and estimates are population-weighted.

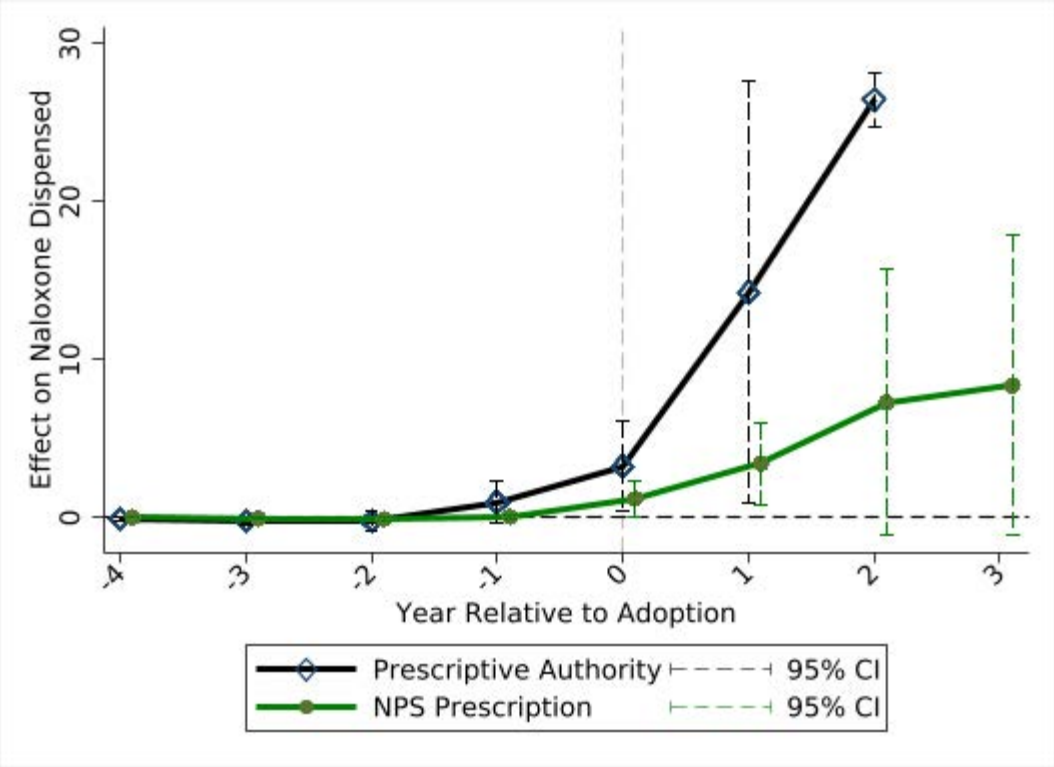


Figure A4: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, No Covariates

Notes and sources: IQVIA data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects and month fixed effects. The parameters associated with these variables are estimated using untreated (no standing order or prescriptive authority) observations only. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

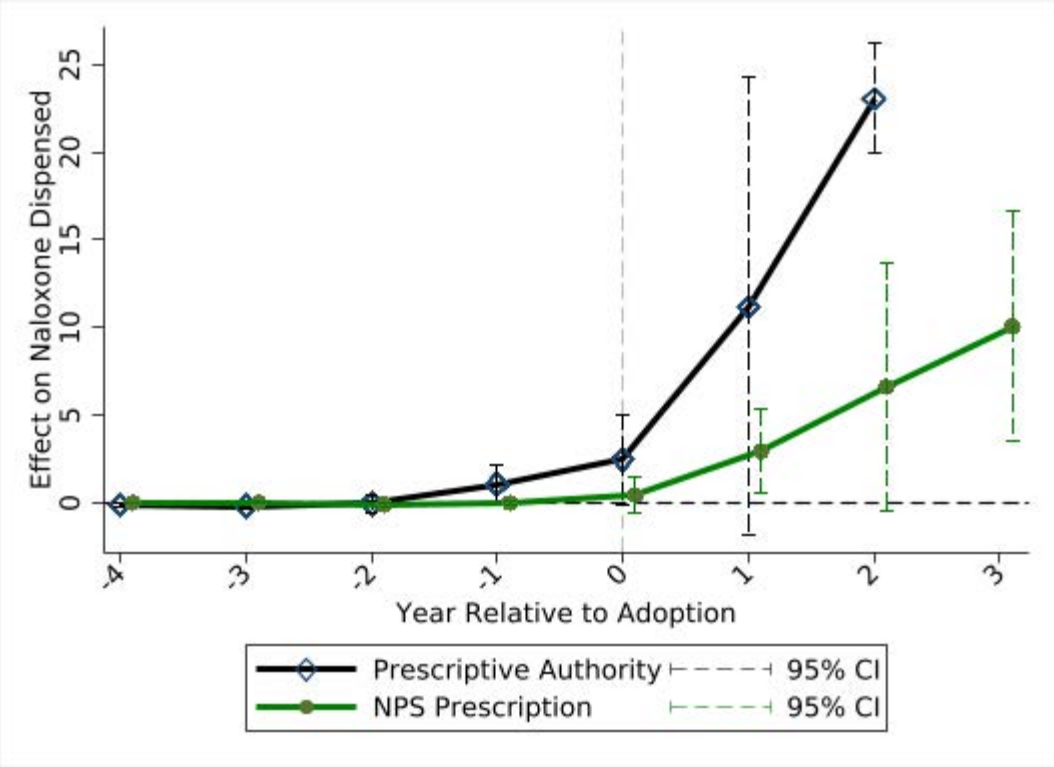


Figure A5: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Additional Covariates Predicting Illicit Opioid Deaths

Notes and sources: IQVIA data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, month fixed effects, and covariates. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. In addition, we include the state non-medical OxyContin misuse rates for 2004-2009, interacted with year indicators, to model the transitions to illicit opioids. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The parameters associated with these variables are estimated using untreated (no standing order or prescriptive authority) observations only. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

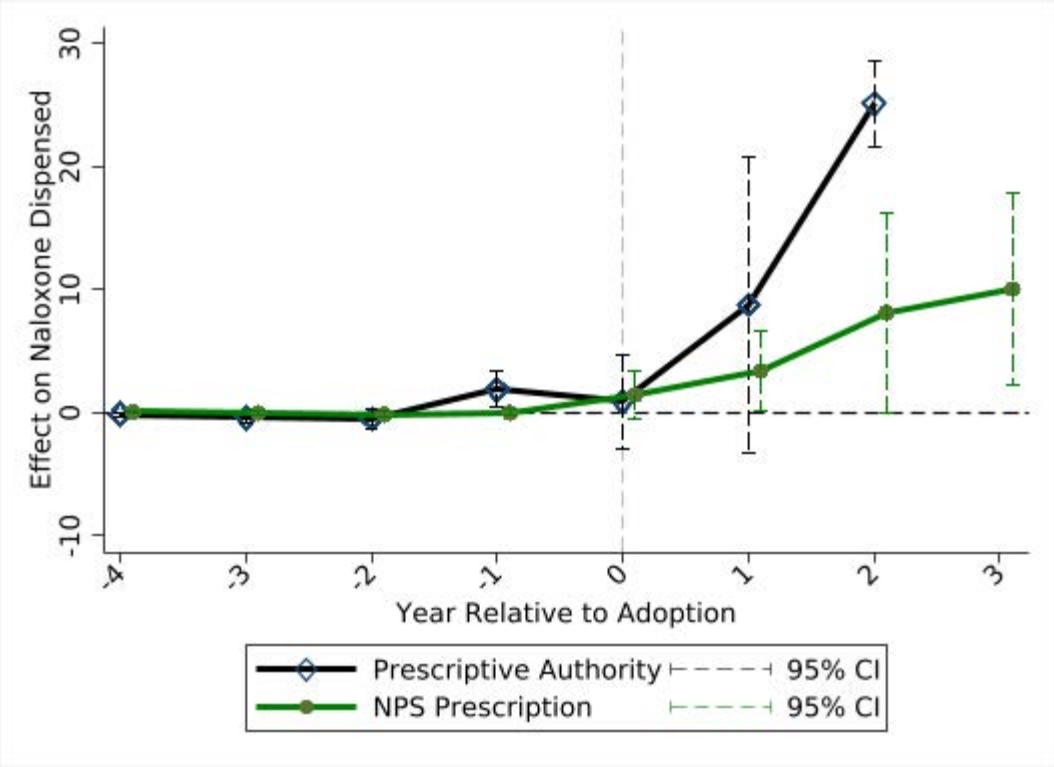


Figure A6: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Unweighted

Notes and sources: IQVIA data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, month fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no standing order or prescriptive authority) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are unweighted.

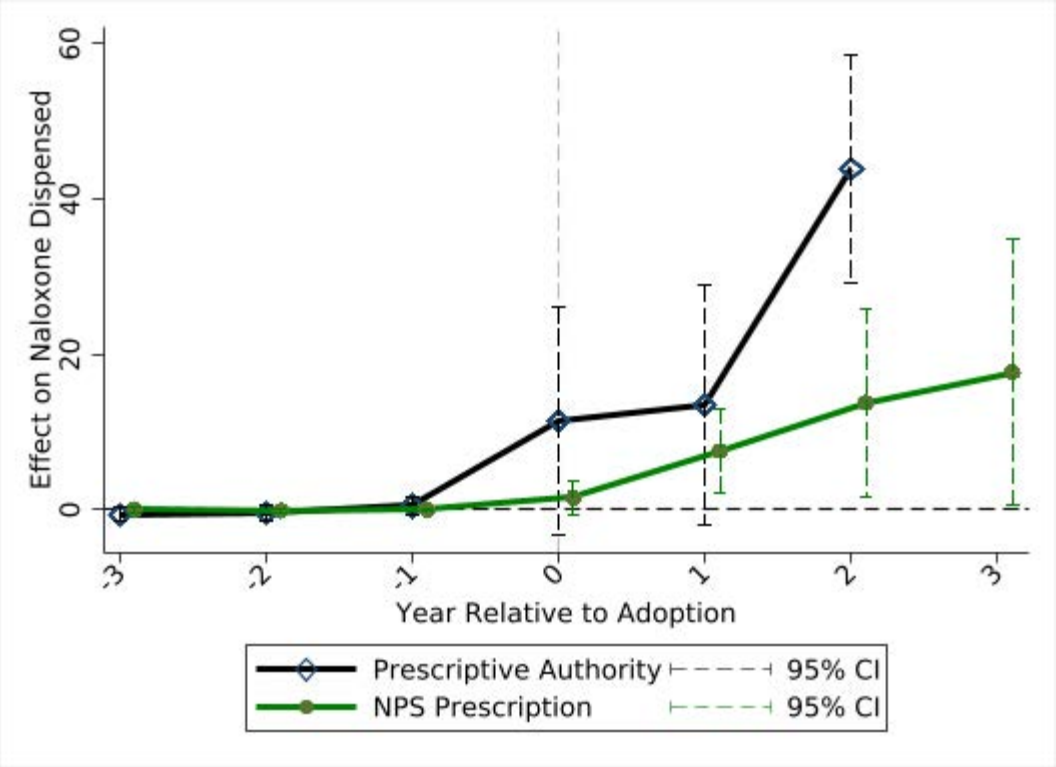


Figure A7: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, 2014-2018

Notes and sources: IQVIA data. N=792. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, month fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no standing order or prescriptive authority) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -3 estimate refers to 3 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted. We exclude California, Kentucky, New Jersey, North Carolina, Oregon, and Vermont from this analysis since they are always-treated during this shorter sample period.

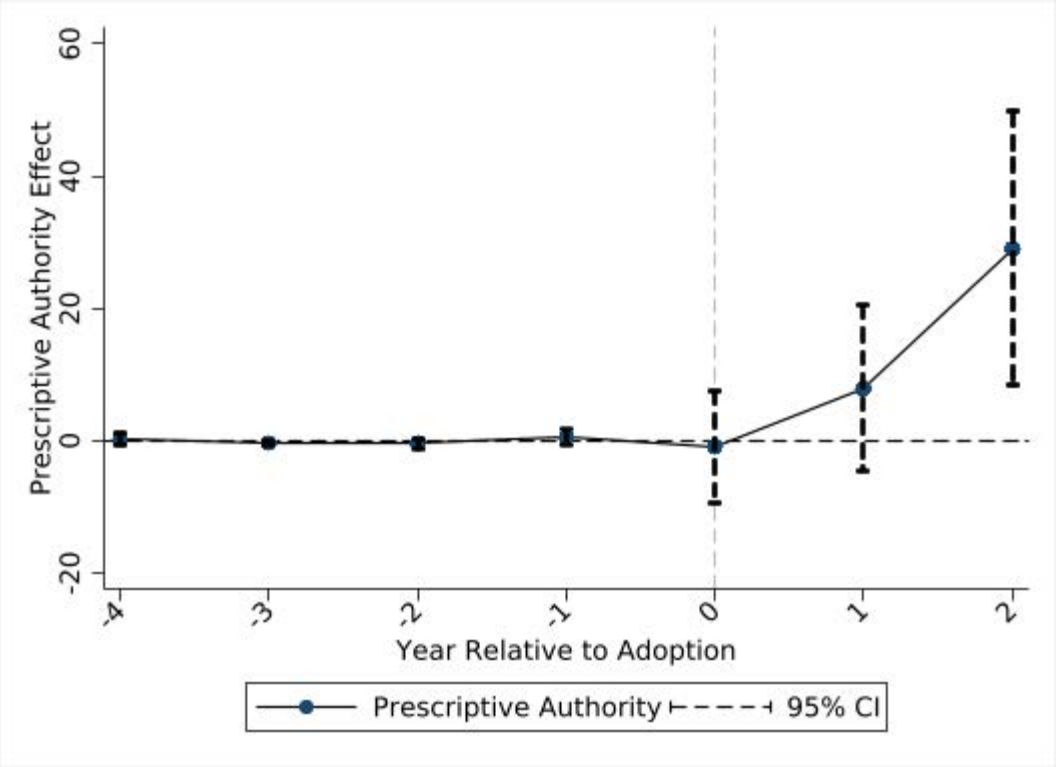


Figure A8: Effects of Pharmacist Prescriptive Authority Laws Relative to Non-Patient-Specific Prescription Laws on Pharmacy-Based Naloxone Distribution Rates

Notes and sources: IQVIA data. N=619. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. All models and estimates are population-weighted. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. The analysis matches each pharmacist prescriptive authority state with NPS prescription law states adopting in the same quarter. We exclude Washington D.C., Maine, and Oklahoma from this analysis since no states adopted NPS prescription laws at the same time. New Mexico (2014q2) is matched to Georgia, Minnesota, and Rhode Island. Connecticut and Idaho (2015q3) are matched to Alabama, Arkansas, Louisiana, Mississippi, New Hampshire, Ohio, Texas, and Washington. North Dakota and Oregon (2016q2) are matched to Alaska, Iowa, and Utah. Wyoming (2017q3) is matched to Kansas. Again, the only criterion for matching is whether the states adopted their policies in the same quarter. We include time fixed effects which vary by "cohort" (when the law was effective) such that each pharmacist prescriptive authority state is only compared to states adopting NPS prescription laws at the same time.

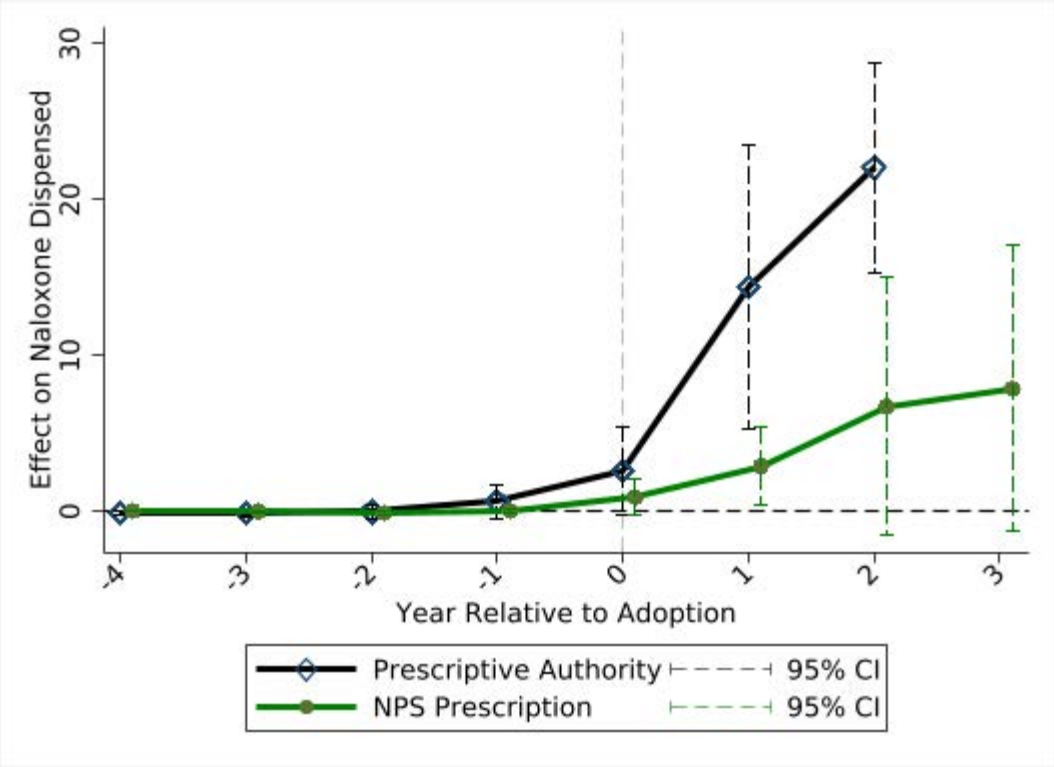


Figure A9: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Sensitivity to the Coding of Oklahoma's NAL policies

Notes and sources: IQVIA data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted. These estimates differ from the main estimates because we recategorize Oklahoma's NAL policies as described in the text.

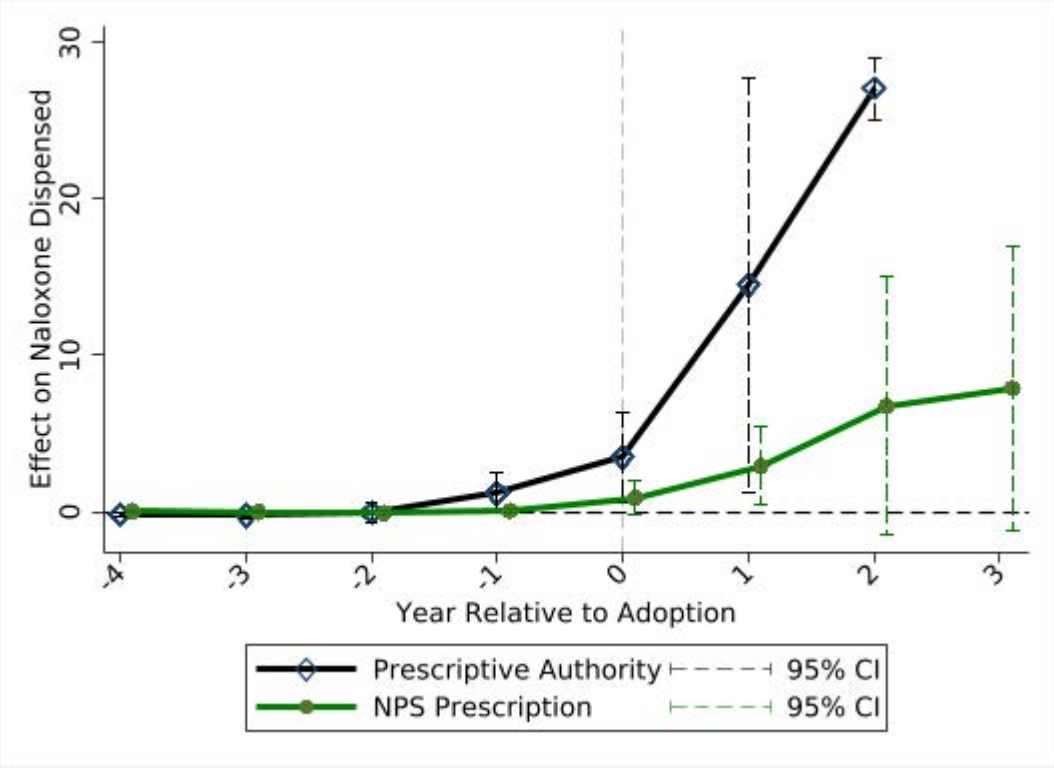
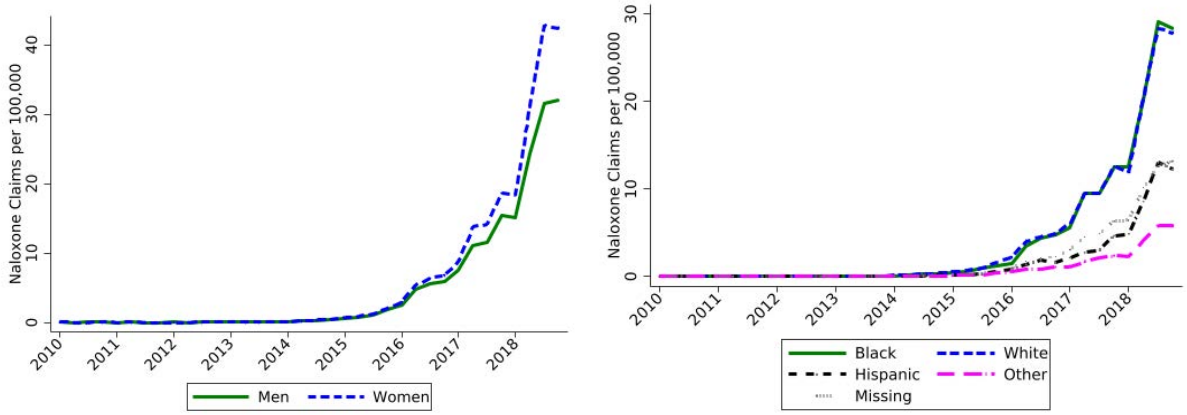


Figure A10: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Dropping Oklahoma

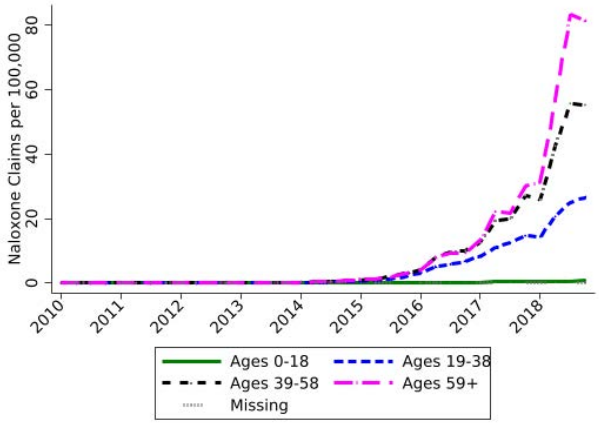
Notes and sources: IQVIA data (2010q1-2017q2). N=1470. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted. These estimates differ from the main estimates because we drop Oklahoma.

APPENDIX B



A. Sex

B. Race/Ethnicity



C. Age

Figure B1: Trends in Naloxone Pharmacy Fills per 100,000 by Demographics

Notes and sources: Symphony Health data.

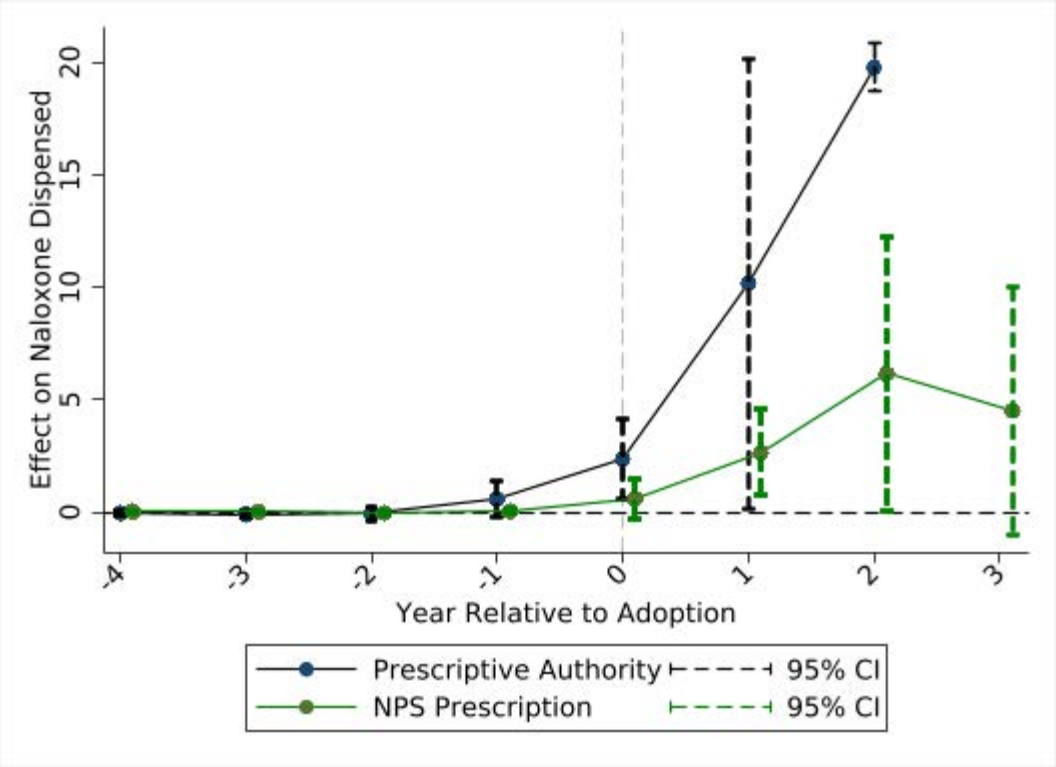


Figure B2: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no standing order or prescriptive authority) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

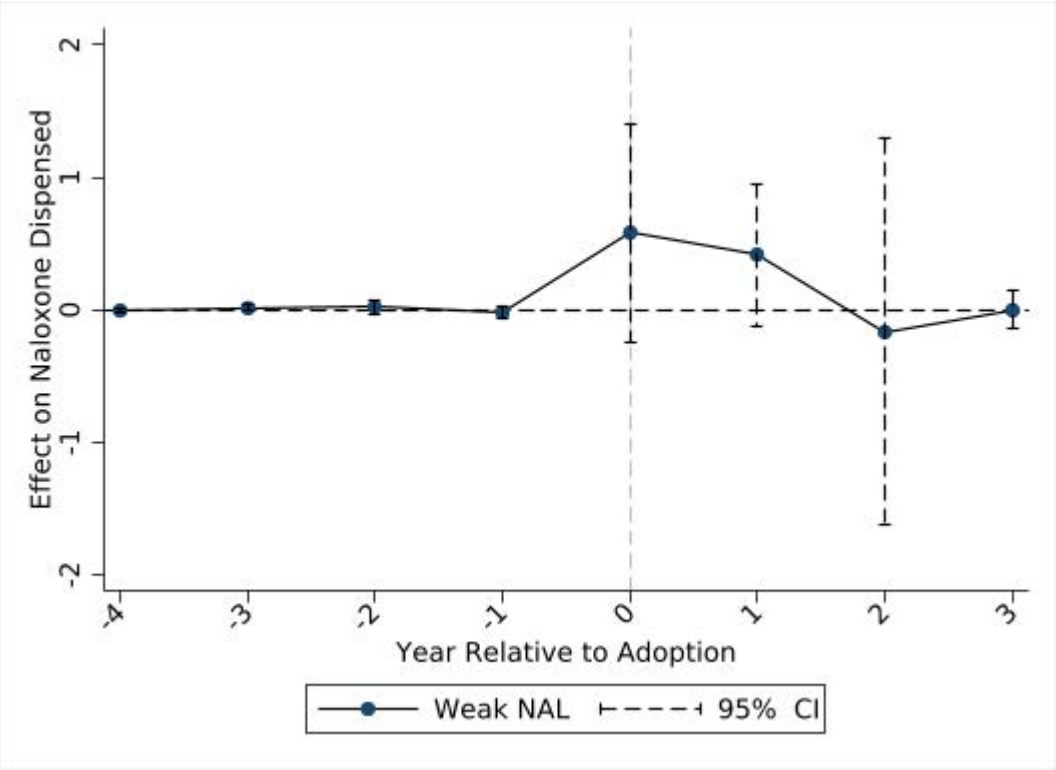


Figure B3: Effects of "Weak" NALs on Pharmacy-Based Naloxone Distribution Rates

Notes and sources: Symphony Health data. N=1021. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. States are excluded once they adopt a non-patient-specific prescription law or pharmacist prescriptive authority law. States with NALs before 2010 are also excluded since they are always-treated. We further limit the sample to end in 2017q2 since all states had adopted some type of NAL by July 2017. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. All models and estimates are population-weighted.

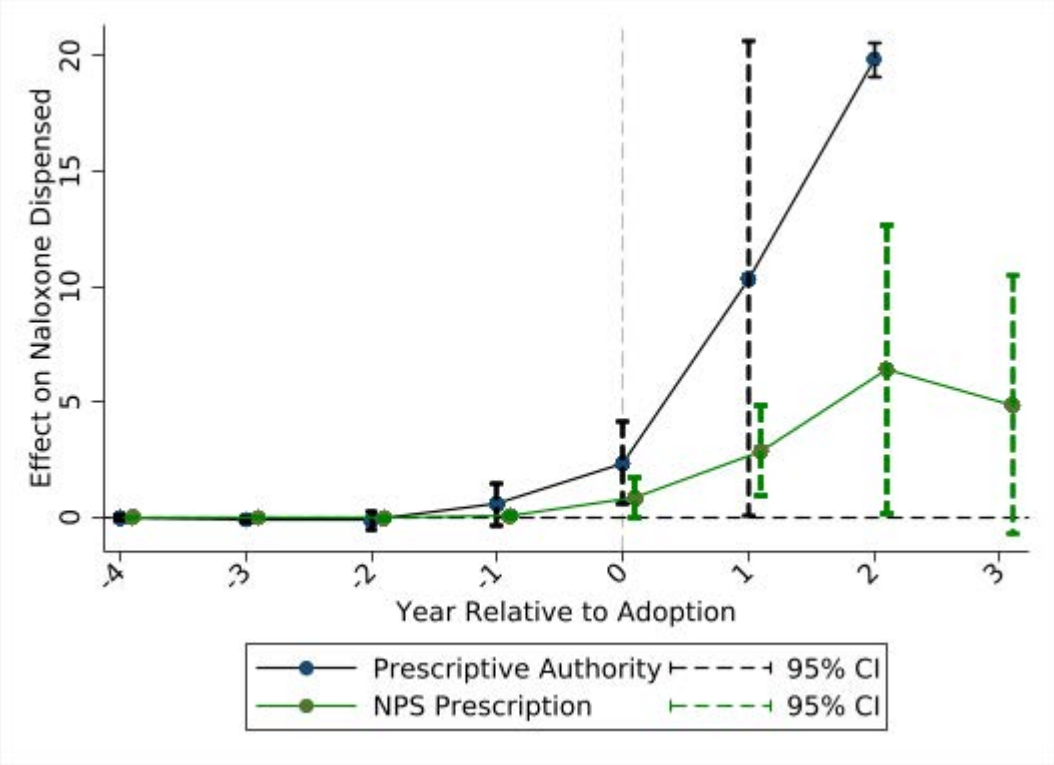


Figure B4: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, No Covariates

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects and quarter fixed effects. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

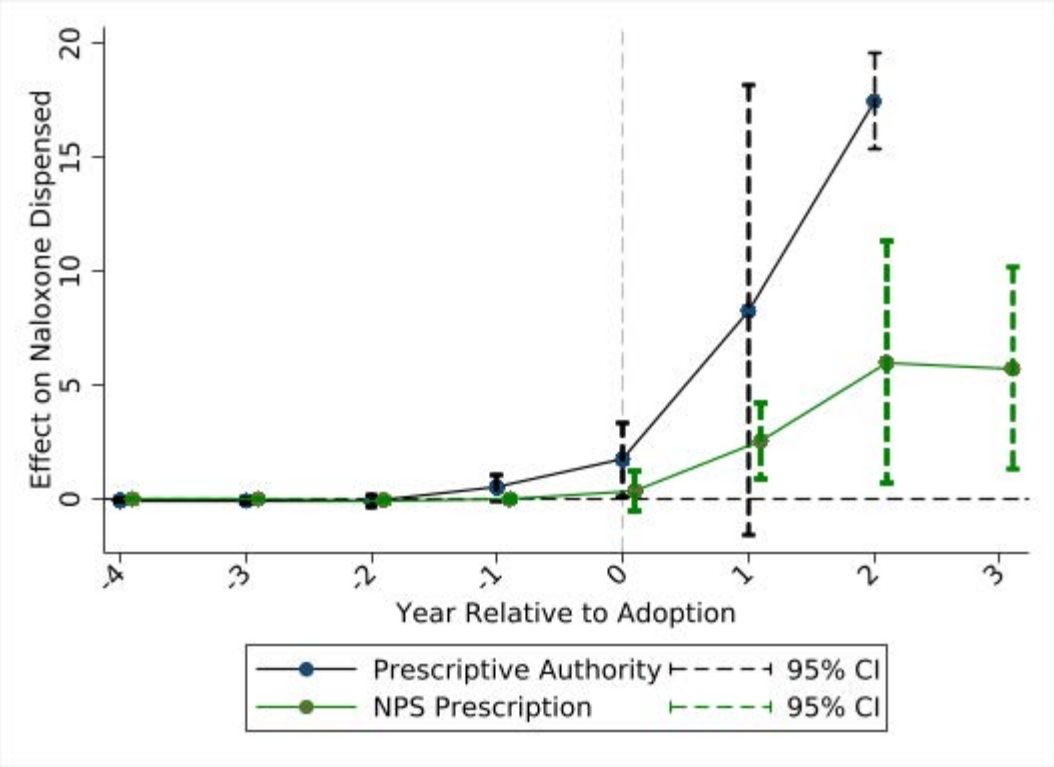


Figure B5: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Additional Covariates Predicting Illicit Opioid Deaths

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. In addition, we include the state non-medical OxyContin misuse rates for 2004-2009, interacted with year indicators, to model the transitions to illicit opioids. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted.

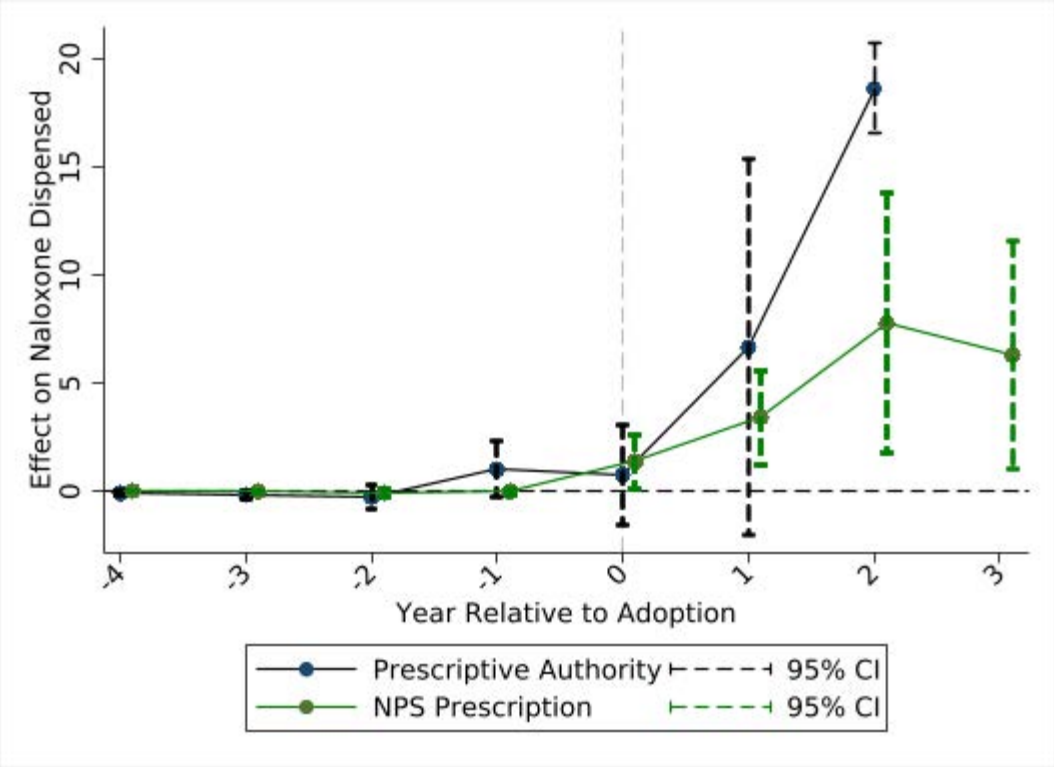


Figure B6: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Unweighted

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are unweighted.

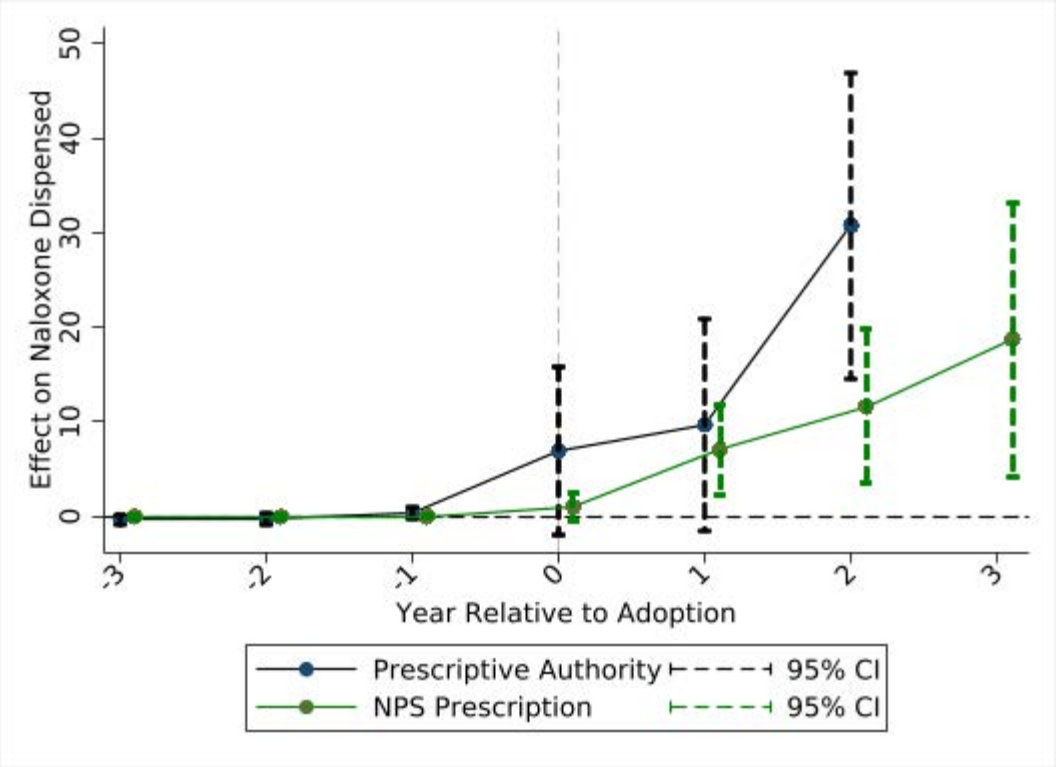


Figure B7: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, 2014-2018

Notes and sources: Symphony Health data. N=792. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -3 estimate refers to 3 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted. We exclude California, Kentucky, New Jersey, North Carolina, Oregon, and Vermont from this analysis since they are always-treated during this shorter sample period.

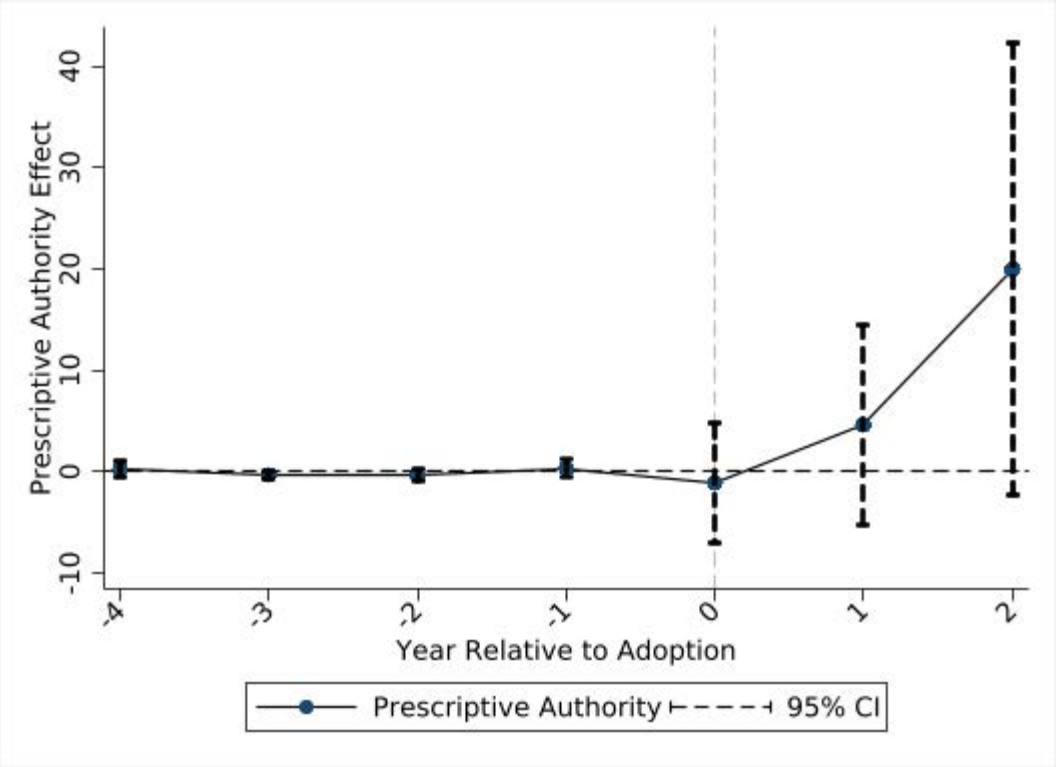


Figure B8: Effects of Pharmacist Prescriptive Authority Laws Relative to Non-Patient-Specific Prescription Laws on Pharmacy-Based Naloxone Distribution Rates

Notes and sources: Symphony Health data. N=619. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. All models and estimates are population-weighted. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. The analysis matches each pharmacist prescriptive authority state with NPS prescription law states adopting in the same quarter. We exclude Washington D.C., Maine, and Oklahoma from this analysis since no states adopted NPS prescription laws at the same time. New Mexico (2014q2) is matched to Georgia, Minnesota, and Rhode Island. Connecticut and Idaho (2015q3) are matched to Alabama, Arkansas, Louisiana, Mississippi, New Hampshire, Ohio, Texas, and Washington. North Dakota and Oregon (2016q2) are matched to Alaska, Iowa, and Utah. Wyoming (2017q3) is matched to Kansas. Again, the only criterion for matching is whether the states adopted their policies in the same quarter. We include time fixed effects which vary by "cohort" (when the law was effective) such that each pharmacist prescriptive authority state is only compared to states adopting NPS prescription laws at the same time.

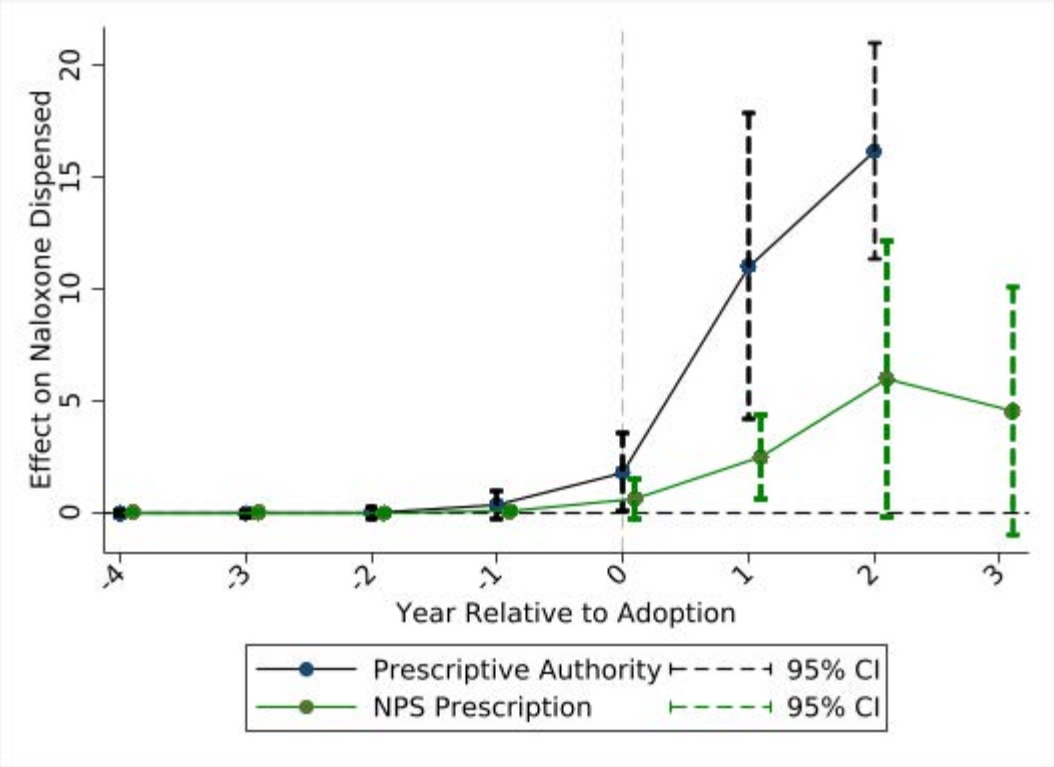


Figure B9: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Sensitivity to the Coding of Oklahoma's NAL policies

Notes and sources: Symphony Health data (2010q1-2017q2). N=1500. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted. These estimates are based on analyses that recategorize Oklahoma's NAL policies to have pharmacist prescriptive authority as of November 2014.

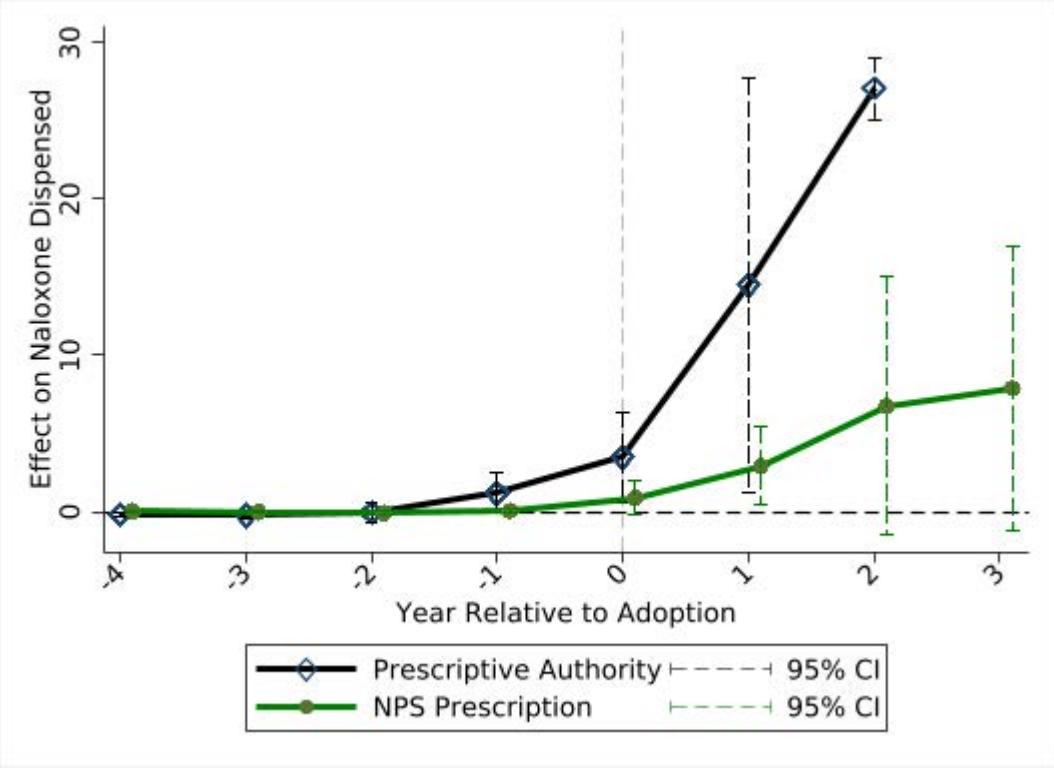


Figure B10: Effects of Non-Patient-Specific Prescription and Pharmacist Prescriptive Authority NALs on Pharmacy-Based Naloxone Distribution Rates, Dropping Oklahoma

Notes and sources: Symphony Health data (2010q1-2017q2). N=1470. 95% confidence intervals are adjusted for state-level clustering. 2sDID used for estimation. We condition on state fixed effects, quarter fixed effects, and covariates. The parameters associated with these variables are estimated using untreated (no NPS prescription or pharmacist prescriptive authority law) observations only. The covariates include the share of the population that is White, ages 0-18, ages 19-38, and ages 39-58 plus policy variables. The policy variables are Good Samaritan Laws, Medicaid expansions under the ACA, and Pain Clinic Laws. The -4 estimate refers to 4 years or before; the 3 estimate refers to 3 years or after. For prescriptive authority, the 2 estimate refers to 2+. See text for definition of the estimates. All models and estimates are population-weighted. These analyses drop Oklahoma.